ICE OR WATER

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ANOTHER APPEAL TO INDUCTION FROM THE SCHOLASTIC METHODS OF MODERN GEOLOGY

BY

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CONTENTS.

CORRECTIONS.

Page 8 lines 18 and 19. Insert commas after "devised" and "inquiry".
„ 13 line 8. For "marks" read "mark".
„ 13 „ 39. For "glaciers" read "icebergs".
„ 18 „ 8. For "*ibid.*" read "*Q.J.G.S.*".
„ 23 „ 30. For "Brittlebant" read "Brittlebank".
„ 25 „ 31. After "so" insert "found".
„ 45 lines 30, 33 and 38. Substitute "æ" for "e" in "paleontology" and "paleolithic".
„ 58 line 9. Erase "they".
„ 67 „ 37. Insert "clay" after "that".
„ 85 „ 4. For "it" read "the latter".
„ 85 „ 13. Erase "the".
„ 87 „ 22. For "deer" read "*Cervus*".
„ 96 „ 11. For "*Fraxirus*" read "*Fraxinus*".
„ 117 „ 30. For "he" read "Mr. Kendall".
„ 121 „ 17. Insert "which" after "but".
„ 126 „ 4. For "Holz" read "Holst".
„ 128 „ 7. Insert "prolonged" after "general".
„ 150 „ — Insert inverted commas at the beginning and end of the third paragraph.
„ 159 „ 29. Insert "south of the Thames" after "them".
„ 165 „ 27. Erase "of".
„ 181 „ 21. Insert a comma before "couvre".
„ 187 „ 8. For "Dupont's" read "Dumont's".
„ 193 „ 5. For "level" read "levels".
„ 195 „ 20. For "Pohatcong" read "Potomac".
„ 201 „ 34. For "spread" read "spreads".
„ 224 „ 30. For "and the patches" read "from the patches being".
„ 232 „ 16. For "Prof." read "Sir Archibald".
„ 243 „ 31. For "has" read "have".
„ 248 „ 19. For "glacier" read "glaciers".
„ 252 „ 36. For "these" read "them".
„ 255 „ 40. Insert "stones from" before "river".
„ 264 „ 31. For "had" read "have".
„ 266 „ 29. Insert "on the" before "coast".
„ 270 „ 4. For "Trevyham" read "Trevelyan".
„ 272 „ 24. For "Llugvig" read "Llugry".
„ 274 „ 21. For "Prof." read "Mr.".

Page 276 line 24. For "Bermot" read "Penck".
" 292 " 18. Erase "and".
" 311 " 33. For "parts" read "facts".
" 313 lines 14 and 17. Insert inverted commas before "many" and after "curves".
" 358 line 26. For "seems" read "seem".
" 370 " 14. Insert "so" after "be".
" 372 " 4. For "its" read "it".
" 375 " 10. For "south-south-west" read "south-south-east".
" 379 " 35. For "their" read "these".
" 387 " 34. After "south" insert "and towards instead of from the mountains".
" 400 " 26. For "a cañon" read "cañons".
" 420 " 24. Insert a comma after "them".

CHAPTER X.

SOME *A PRIORI* ARGUMENTS AGAINST THE GREAT ICE AGE.

"In hoc genere et naturali et honesto duo vitia vitanda sunt: unum, ne incognita pro cognitis habeamus iisque temere assentiamus; quod vitium effugere qui volet (omnes autem velle debent,) adhibebit ad considerandas res et tempus et diligentiam," etc.—Cicero, *Offic.*, i., 6.

In the preceding volume I have endeavoured to analyse, with such fairness and skill as I am capable of, the problem of the origin and cause of the surface features of the earth. I have tried to show that the view current among orthodox geologists is an impossible one, and that these surface features are not only to a very slight extent due to erosion and denudation by subaerial agencies or by rivers and the sea, but are largely due to movements of the earth's crust, caused either by its shrinkage when cooling or by more positive and aggressive forces which have induced local phenomena of a marked kind. To the action of subterranean forces I have assigned the formation of mountains and valleys, of fiords and lakes, thus following in the footsteps of my masters, the geologists of a former generation, and have assigned to erosion and diurnal denudation the polishing or smoothing or rounding of a mere surface cuticle of the solid ground in certain places, and the deposit upon it in others of sheets and mantles of gravel, sand and clay.

My purpose in analysing this problem has not been entirely to join issue with the current theories in regard to denudation adopted by the Huttonian and Lyellian school of geologists, but also to advance my arguments against the glacial theory.

In the earlier chapters of this book I tried to analyse the various theories which have been current to account for an ice age, and I have ventured to conclude that with one exception they have ceased greatly to interest geologists,

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since they have all been shown to be inconsistent with the laws of matter or force, and to involve impossibilities of logic, of mathematics or of mechanics, or to be based on quite impossible and transcendental premises.

The one theory which still has a respectable following, not in this country nor on this side of the Atlantic, but in America, is the so-called *epeirogenic* theory of an ice age. It is based on a very plausible and true idea, namely, that the low temperature of high latitudes is very largely caused by and dependent upon the high level of the land there, and if we could secure a sufficiently elevated mass of land in high latitudes in so-called glacial times we should have done a good deal to explain the glacial theory.

It has been a real initial difficulty to any theory of the kind that the notion which commended itself to Agassiz, Torell and other early champions of the glacial nightmare that the ice of the glacial period culminated in the Arctic regions is no longer tenable. From all sides comes the information, and I have already set it out in an earlier chapter, that the notion of polar ice-caps or of a culmination of ice at or near the poles is quite out of the question, since the course of both the boulders and the striæ that are supposed to attest the handiwork of the ice in glacial times is northwards towards the pole in Norway and Finland and in North America, which it could not have been if the culmination of the ice had been there. We must, therefore, exclude the poles as the culminating points of the ice in the ice age.

The critical foci of the ice movements, from which it must have started and spread, must assuredly have been the points from which the drifts of the boulders and the direction of the striæ diverge. It is there, therefore, as I have said, that the supposed ice-sheets must have culminated so as to give an impulse to the movement of the mass of ice, for no one believes now that without some impulse of some kind the ice could move at all.

In the northern hemisphere the main centres and foci of the phenomena are—in Europe the comparatively low lands of Northern Sweden, Dalecarlia, etc., and of Scotland; in North America the uplands of Labrador, and in South America the southern extremity of the great Cordillera.

If the epeirogenic theory, therefore, affords a *vera causa* of an ice age, it is in very high latitudes that we must search for evidence of much greater elevation of the soil in former times and of much greater precipitation. It is a grave difficulty in the way of such a theory that the direct evidence of epeirogenic movements in these districts in both hemispheres is wanting. In the very first paper I ever wrote, which was afterwards incorporated in the *Arctic Manual*, I ventured to argue (and I think I showed) that wherever we can apply tests the land in high latitudes is, except in very limited localities, rising and not sinking. Any areas of sinking which have been traced there are very local, whereas when we examine Greenland or Spitzbergen or Norway or North America or Scotland the raised beaches, the bones of stranded whales, the drift wood and the stratified sands, etc., at high levels offer one consistent story, namely, that not only has the land risen since so-called glacial times, but it has continuously risen and without intervals of sinking, so that if we are to follow evidence and not conjecture, we can hardly doubt that the level of the land was much lower in former times in high latitudes than it is now, and by former times I mean before and during the deposition of the drift.

Prof. James Geikie has always been a determined opponent of the epeirogenic theory of an ice age. He does not dispute that the elevation of the land in higher latitudes would increase the severity of the climate. He says: "Far above our heads at a less or greater elevation, according to latitude, an Arctic climate prevails. One cannot doubt, therefore, that if a land surface were only sufficiently uplifted it would reach the snow line and become more or less extensively glaciated; but," he continues, "I have never been able to meet with any evidence in favour of the postulated earth movements. Having carefully studied all that has been advanced of late years in support of the hypothesis in question, I find myself more than ever constrained to oppose it; not only because it is grounded on no basis of fact, but because it altogether fails to explain the conditions that obtained in pleistocene and post-glacial times." He then proceeds to examine the position in some detail. "It has been maintained," he says, "that at the advent of the glacial period

vast areas of Northern and North-Western Europe, together with enormous regions in the corresponding latitudes of North America, stood several thousand feet higher than at present. But when we ask what evidence can be adduced to prove this we get no satisfactory reply. We are simply informed that a glacial climate must have resulted from great elevation, and that the latter, therefore, must have taken place at the beginning of the glacial period." He then refers to Mr. Upham's arguments based on the fiords being submerged valleys cut out during the former upheaval of the land, and he contends that these fiords are of great geological antiquity on both sides of the Atlantic. If such be the case it is obvious that the origin and existence of the fiords have no bearing whatever on the problem of the glacial climate and its cause, and he argues that they tell against and not in favour of Upham's conclusion, since they point to the fact that when the land was elevated, as supposed, the fiords were occupied by rivers and not by glaciers. Whatever value this last argument may have in itself—and I do not accept it—it seems to me to be conclusive as against Mr. Upham and his friends, who treat fiords as valleys of erosion. Mr. Geikie then turns to other facts and says: "We are not without direct evidence as to the geographical conditions that obtained in the ages that immediately preceded the pleistocene period. The distribution of the pliocene marine beds of Britain entitles us to assume that at the time of their accumulation our land did not extend quite so far to the south and east as now. In America the beds on the same horizon seem to show that the North American continent was not less extensive than now. But without going so far back as pliocene times, we meet with evidence almost everywhere throughout the maritime regions of the glaciated areas of Europe and North America to show that immediately before these tracts became swathed in ice (say rather before the distribution of the drift—H. H. H.) the geographical conditions were much the same as at present. The presence of shelly boulder clays in various parts of our islands, and the similar occurrence of marine and brackish water shells in and underneath 'the diluvium' of North Germany, etc., prove clearly enough that just before the coming on of glacial conditions neither Britain nor the present maritime

lands of the continent were far removed from the sea. . . . We are led to similar conclusions with regard to the geographical conditions of North America at that time from the occurrence of marine shells in the boulder clays of Canada and New England. . . . While we have no evidence of widespread elevation having coincided with glacial conditions, proofs of subsidence are almost everywhere associated with the glacial phenomena of the maritime districts of North America and Europe. Raised beaches and marine deposits are traced on the coasts of North America, from an elevation of fifty feet or so in South New England up to 75 to 100 feet near Boston; of 200 feet or thereabouts in Maine; of 520 feet in Montreal; of 1,500 feet in Labrador, and of about 1,000 to 2,000 feet in the Arctic regions. None of the raised beaches of glacial age met with in Europe reaches such an elevation as these last, the highest being met with in Norway at 580 feet or thereabouts. Marine shells occur in the glacial series of Scotland at a height of 510 feet, but the highest raised beach of the period does not exceed 100 feet in elevation." Geikie concludes from these facts that considerable submergence of the land took place in glacial times ("The Glacial Period and the Earth Movement Hypothesis," by J. Geikie, *Trans. Vict. Inst.*, 1892, pp. 16-26). The facts here summarised, and which will occupy us in greater detail later on, are nowhere, so far as I know, disputed. They are too palpable. Mr. Warren Upham, the archpriest of the epeirogenic theory, plainly sees the difficulties, and I have already quoted his very frank statements in regard to them (vol. i., pp. 136, 137). He admits that the evidence of the shells and of the so-called glacial clays, etc., is desperately against him, since it is difficult, if not impossible, for those who hold his view to separate their deposition from the culmination of the so-called ice age; but, notwithstanding this evidence and this difficulty, he nevertheless falls back on Dana's notion, that the existence of the fiords proves the former elevation of the land, as having an overwhelming preponderance of authority in such a discussion. Dana had laid it down that the fiords of Greenland, Norway and North America are merely valleys eroded by streams during a formerly greater elevation of the land in high latitudes, and this view has been widely accepted as proven, and as having the

authority of the Syllabus among Roman Catholics. I have devoted a considerable space to the analysis of it in the three previous chapters, and claim to have shown that it is quite worthless as a scientific induction. Instead of being valleys eroded by rain or ice the fiords in question are simply transverse rifts caused by the elevation of the high lands where they occur. They do not testify in any way to cutting back by rivers when the land was at a higher level, but were the actual and necessary products of the last upheaval of these lands. So that in appealing from the evidence of the raised beaches and the shells in the low-lying clays to that of the fiords, Mr. Upham is in effect appealing from the facts to an exploded hypothesis.

Even his own friends among the erosionists have questioned the relevancy of Mr. Upham's appeal. They have argued that he has not shown that the fiords were not much older or much younger than the ice age, and has arbitrarily assigned them to that period. The fact is, however, that the use of these fiords as a test of whether the land is rising or not is quite a futile process, because, as we have seen, they are not the results of erosion at all, but of fracture. With the disappearance of this prop the whole epeirogenic theory falls to the ground, and we are constrained by the remaining evidence not only to deny that the land was formerly higher where the ice is supposed to have culminated, but to assert that it was lower, and that, consequently, the climate, instead of being more severe, must have been, *ceteris paribus*, less so. If the epeirogenic theory, therefore, were a *vera causa* of an ice age, such a period ought to be now in progress.

In all these areas we have terraces at various heights. We have raised beaches with marine shells at various heights; we have deposits of stratified marine sands with marine shells at various heights. We have, in fact, all the evidences which we naturally turn to when we want to know whether a land surface has been rising or not.

Geikie uses a final argument against the epeirogenic theory, which has not, so far as I know, been hitherto met, namely, that granting that such an elevation of the land took place as is demanded by the champions of that view, of which, as we have seen, there is no evidence, it would not account for

the facts. In order, he contends, to explain those facts in the manner they are interpreted by the glacialists, namely, by the intervention of a great ice age, we must not only postulate a much greater precipitation over the glaciated areas than exists now, but the geographical distribution of glacial, fluvio-glacial and other pleistocene deposits leads to the conclusion that in glacial times a wholesale displacement of climatic zones took place; and he proceeds to say: "I deny that the conditions that prevailed in pleistocene times can be accounted for by elevation and depression. . . . Were North-Western Europe and the corresponding latitudes of North America to be upheaved by 3,000 feet, and a land passage to obtain between the two continents by way of the Faroe Islands, Iceland and Greenland, how would the climate be affected? It is obvious enough that under such changed conditions the elevated lands in higher latitudes might well be subjected to more or less extensive glaciation. Norway would become uninhabitable, and glaciers might well appear in the mountain valleys of Scotland. But it may be doubted whether the climate of France and Spain, or the corresponding latitudes of North America, would be much affected. For were a land passage to appear between Britain and Greenland, no Arctic current would flow into the North Atlantic, while no portion of the Gulf Stream would be lost in the Arctic seas. The North Atlantic would then form a great gulf round which a warm ocean current would circulate. The temperature of that sea, therefore, would be raised, and the prevailing westerly and south-westerly winds of Europe would be warmer than now. However much warm moist winds might increase the snowfall in North Britain and Scandinavia, we cannot suppose they could have much influence in Central and Southern Europe and in North Africa, and still less could they affect the climate of Asia Minor and the mountainous regions of the Far East, in most of which evidence of extensive glaciation occurs. And how, we may ask, could the postulated geographical change bring about the glaciation of the mountainous tracts on the Pacific seaboard? In fine, we may conclude that however much the geographical changes referred to might affect North-West Europe and North-East America, they are wholly insufficient to account for the glacial pheno-

mena of other regions. The continuous research of recent years has shown that the lowering of temperature of glacial times was not limited to the lands which would be affected by any such elevation as that we are considering. A marked and general displacement of climatic zones took place over the whole continent of Europe, and similar changes supervened in North America and Asia. Are we, then, to suppose that all the lands within the north hemisphere were extensively and contemporaneously upheaved?" (*op. cit.*, pp. 20, 21). This is very forcible, and to those who accept the glacial postulate as generally received it ought, it seems to me, to be conclusive.

This conclusion in regard to the epeirogenic theory is of very considerable importance to the issue we are discussing, for it disposes of the last prop upon which the seekers after a reasonable cause for an ice age have depended for support. So far as I know there is no other theory of any kind which the ingenuity and skill of a whole school of geologists has devised (I mean the glacial champions) during half a century of eager inquiry which will stand criticism. Every one of them crumbles away when analysed by the precise methods of mathematical or mechanical induction or when put to the test of experimental verification. At present, therefore, the glacial geologists stand before the world as the champions of a theory which they have pertinaciously pressed upon everybody in season and out of season, which has been very widely, nay, almost universally, accepted, and yet whose premises are incapable of verification by any known method. A vast series of phenomena are attributed to a cause which cannot be explained by any known method. Astronomy, physics and meteorology have all been appealed to in turn, and all of them refuse to give any countenance to a great ice age. If such an age ever existed it is clear that its cause is quite unknown to us, and, so far as I see, is out of the reach of scientific analysis. This is undoubtedly an extraordinary position. The champions of uniformity, as taught by Hutton and Playfair, Lyell and Ramsay, in their efforts to explain perhaps the most stupendous event in geological time, have no better clue to offer us than a cause which is quite unknown to experience—that is to say, a purely transcendental cause. It is true that

we are constrained to believe in many effects for which we cannot always assign an adequate cause. We do not know what ultimately causes the phenomena of electricity or thought or gravitation, for instance, and we are content to say so; but that is not the kind of issue involved in the present case. In the present case the phenomena are all admitted as they are in the case of electricity or thought or gravity, and we should be fairly content if those who studied them confessed their inability to find an adequate cause for them. What we complain of is that they do profess to find a cause, and that their cause is one that will not stand the test of analysis, since it cannot be equated with the phenomena at all, and is in itself inexplicable according to any known laws of matter or force. Such an hypothesis seems to me to be a mere phantasm, for it has no inductive basis. Those immature philosophers of the nursery who hold the moon to be made of green cheese, and point to the spots on its face as evidence of the burrowing of mice, have as great a right to claim a scientific basis for their theory as the solemn philosophers who appeal to an ice age, which they cannot show to be consistent with any of the known operations of nature. This is surely a fatal *a priori* objection to the whole theory.

A second one, as I have repeatedly urged, is that not only has no efficient cause for an ice age been as yet suggested, but that all the important arguments of the ice-men involve appeals not only to unverified powers and qualities in ice, but to powers and qualities which, so far as experiment and other evidence go, are quite inconsistent with the nature of ice. To the genuine glacialists (I mean to men like Croll[1]), who tried to treat the problem on scientific lines, it was more or less obvious that their conclusion could not be reached, unless they attributed to ice virtues which no experiment had verified in it; and the ice which he argued about, therefore, and discussed and appealed to was a transcendental ice having molecular movements and qualities quite unlike other mundane substances. From Croll this transcendent

[1] It must be remembered that as late as 1870 Croll writes: "The ice of a glacier is now almost universally believed to be not a soft plastic substance, but a substance hard, brittle and unyielding" (*Phil. Mag.*, xl., p. 153).

ice passed on (ready made) to the geological champions of an ice age, and when the latter changed their front and accepted Forbes' definition of ice as a viscous substance and nothing more, they did not seem to have seen that this new definition was entirely inconsistent with their previous arguments and conclusions, which were based not on the ice for which the Almighty is responsible, but on the particular kind of ice distilled from the thought of a most erratic and daring speculator.

When Croll and his followers talk and write about the vast mounds of ice many miles thick as the instrument with which they would work, they never stop to inquire about what would occur to most other people as an elementary factor in the problem, *viz.*, the amount of ice which can be piled up in a heap without causing it to crush, and thus to dissipate its energy. When they speak of ice carrying loads of stones either on its back or beneath its foot for hundreds of miles they do not try to analyse how the problem is to be compassed; whence a sufficient force is to come from, or how it is to be sustained, while they eagerly postulate some new quality of ice unknown to experiment to explain every fresh difficulty in the phenomena. The fact is that, according to all inductive methods of any value, the capacity of the instrument which is appealed to, *i.e.*, the ice, should first be proved and then applied to the elucidation of the problem. When the phenomena of glaciers are quoted against the glacialists they will have none of them. They repudiate them. They reply that the puny glaciers of our time are no measures of the capacity, of the vast potentiality of the enormous ice masses which they deal with, either in degree nor kind. They therefore turn their backs on glaciers altogether and appeal to entirely unknown qualities of ice which their imaginations endow ice-sheets with, and thus create another stupendous, and in fact insuperable, *a priori* objection to an ice age—one, too, which it is particularly arbitrary and indefensible for those to appeal to who claim Uniformity as their scientific inspirer. We who abandoned the study of metaphysics many years ago because we found that we got a very unsubstantial web when the north wind was our warp and the east wind our weft, repudiate that science which is not based on in-

duction but on imaginative postulates. We are content to attribute to ice action what we find by experiments in the laboratory or by watching glaciers that ice can be shown to be capable of, and we dare not, without risk to our scientific sanity, invoke forms of ice which our experience does not know as if we were scholastic philosophers instead of pupils of Bacon and Newton. We know experimentally that a viscous flow in ice as in other substances needs gravitating pressure, which either moves the ice *en masse* or makes its particles roll over each other; and with a substance like ice we know of no other kind of motion.

The movement of ice *en bloc*, as we have seen, is a very limited element and scarcely appreciable, and the glacialists are fairly agreed that in appealing to the movement of ice they are appealing to its viscous movement.

So far as experiment controls our conclusions, it is clear that the motion of the nether layers of the postulated ice-sheets of America and Europe on flat or rolling surfaces would only be possible with such a surface slope of the ice as is inexplainable if we are to believe in ice crushing under certain definite pressures; while even if explainable it would mean that all the sources for the glacial débris (which were *ex hypothesi* deposited by it) would be buried deep in ice and therefore unattainable. The statement, again, that a mass of ice can pluck stones out of its bed (to use Prof. Chamberlin's phrase) when pressing upon it with the weight of many tons to the square inch, and when moving at a pace to which that of a snail is railway speed; that having dug up its own floor under these conditions the stones, etc., thus obtained can travel upwards through the ice while the ice itself is travelling downwards, and in thus overcoming gravity can mount ascents of more than a mile high in a very short space; these things are as credible to some of us as the statement made by the clown to the policeman in the pantomime about the *provenance* of the treasures he has stored in his ample pockets. We cannot understand the arguments nor the mental attitude of those who produce them. The literature of ground moraines and of englacial moraines is steeped full of this kind of logic, and the necessity of appealing to it constitutes another most powerful *a priori* argument against the theory of an ice age

as taught by its prophets. It is comforting to think that the inventors of the theory of englacial drift seem to be disposed to be sarcastic about, and to be willing to exclude as irrational, the appeals of the champions of ground moraines. "A crow ought not to pluck another crow's eyes out," says the French proverb.

Thirdly, as we have seen, Prof. Geikie, like other glacial champions, does not limit himself to transcendental postulates in regard to ice merely. He does not hesitate to invoke climatic and meteorological revolutions, which, so far as we can see, are purely arbitrary, for no cause or explanation of them is vouchsafed. They are thrown at us as mere *obiter dicta*, and just as if it were quite justifiable for any one to appeal to a vast revolution in the general trend of the winds, the rainfall, or the snowfall, by merely turning the magician's wand hither and thither.

The desperate problem these meteorologists *in nubibus* have to face is, without postulating great geographical changes, to explain how on such areas as Dalecarlia, Scotland and Labrador there could possibly be the tremendous precipitation of snow which would be required to make the ice-sheets of the glacialists culminate there. To appeal to this snowfall in these particular regions without explaining how it could be brought about does not seem to me to be science.

The rain and wind makers of the Turks of Central Asia we can understand. They are real medicine-men, and do not disguise their belief in hocus-pocus and the rain stones. The philosophers I am criticising would feel hurt at being put among the prophets of hocus-pocus. I confess I do not know what else they are. One class appeal to the rain stone to bring them rain, while the other class appeal to imaginary climates and weather to create them an ice age. Let us now turn to another test as a touchstone of their arguments.

In a previous chapter, in analysing the various horizons in the geological record which have been supposed at different times to offer traces of glacial action, I ventured with nearly every other geologist known to me to conclude that no adequate evidence of any such traces are present anywhere except at one horizon, namely, that on the frontiers of the Carboniferous and Permian beds.

Thence very considerable evidence is forthcoming, which, if it does not attest phenomena on the gigantic and wide-spread scale which marks the so-called glacial age in pleistocene times, is remarkable enough, not only for the resemblance in the phenomena, but also from their occurring in so many latitudes.

In writing the fourth chapter of this work I postponed the consideration of this horizon and its lessons, and will now condense the new facts I have learnt about it since the publication of my previous work, and I will then draw such conclusions as I think the position warrants. The most remarkable fact about the whole matter, so far as I can judge, is that among the suggested glacial phenomena which occur at this horizon none of them are found in the higher latitudes, where Permo-Carboniferous beds are so widely distributed. For a time it was thought, on the authority of Dahll, that certain conglomerates containing travelled boulders, some of which are striated and which occur in the Varanger Fjord in Norway, belonged to this horizon, but a later examination of these beds (which, by the way, are unfossiliferous) by Reusch and by Strahan make it almost certain that the conglomerate in question is much older than the suggested horizon and that it belongs to Cambrian times (*Quart. Journ. Geol. Soc.*, liii.).

In two other localities (not in high latitudes, but in the north temperate zone where the phenomenon has been noticed) the facts turn out to have been misinterpreted. One of these is in Shropshire and the other in North America, and I will devote a paragraph or two to them.

The first person to attribute the breccias and other boulder deposits of this period in Shropshire to glacial action was Ramsay, who did so as far back as 1855 in a memoir specially devoted to the breccias of Permian age in Shropshire and its borders, which was published in the *Quarterly Journal of the Geological Society*. In this he tells us that he had for some time been struck with the size and angularity of the fragments there and with the marly paste in which they are imbedded, and had even arrived at the conclusion that they were chiefly formed of the moraine matter of glaciers, drifted and scattered in the Permian sea by the agency of glaciers, and in the paper in question he not only professed to show that the icebergs

were of Permian date, but partly also to indicate the district whence the glaciers descended.

In regard to the fragments in the breccias he says: "In spite of exceptional fragments in the Malvern district, they seem to be derived from one set of rocks; they are all enclosed in the same red marly paste, and they are mostly angular or subangular. A well-rounded water-worn pebble is in places of rare occurrence. The surfaces of a great majority of the pebbles are much flattened, numbers are highly polished, and when searched for, many of them are observed to be distinctly grooved and finely striated. The striæ in some are clear and sharp and run parallel to or cross each other at various angles, while in others, though you see their remains, age and surface decomposition have impaired their sharpness and roughened the original polish of the stone. . . . If the lithological character be any guide, the fragments, with rare exceptional pieces, seem to have been derived from the conglomerates and green, grey and purple Cambrian grits of the Long Mynd, and from the Silurian quartz rocks, slates, felstones, felspathic ashes, greenstones and upper Caradoc rocks of the country between the Long Mynd and Chirbury. The south end of the Malvern Hills is from forty to fifty miles, the Abberleys from twenty-five to thirty-five miles, Enville from twenty to thirty miles, and South Staffordshire from thirty-five to forty-five miles distant from that country. The question then arises by what process were so many angular and subangular fragments transported so far, many of them being a foot, and some two, three or even four feet in diameter, the whole in places forming a deposit of several hundred feet in thickness. Why, also, are they angular and not well rounded like the pebbles of the great conglomerate beds of the Bunter sandstone, and why have they flattened sides and often polished and striated surfaces?" Having discussed the plant remains in the beds, and assigned them in consequence to Permian times, Ramsay goes on to discuss the various methods by which the stones in the breccias were deposited, and concludes in favour of ice. This he concludes, first, from the size of the stones, the largest weighing from half to three quarters of a ton; secondly, from their form, few being rounded, most of them being angular or subangular with flattened sides; thirdly, from the fact that

many of them are highly polished and others grooved and freely striated; fourthly, from the hardened cementing mass of red marl in which the stones are thickly scattered, which he thinks may be compared with boulder clay. He holds that it was ice in the nature of glaciers which carried the stones, and if the parent rock, he says, is properly identified, it follows that the ancient territory of the Long Mynds and the adjacent Lower Silurian rocks, having undergone many mutations, at length gave birth to the glaciers, which, flowing down some old system of valleys, reached the level of the sea, and, breaking off into bergs, floated away to the east and south-east and deposited their freight of mud, stones and boulders in the neighbouring Permian seas (*Quart. Journ. Geol. Soc.*, xi., pp. 185-205).

The views here proclaimed by Ramsay are apparently still held by Dr. Geikie. Lyell, who was disposed to accept Ramsay's conclusion, nevertheless wrote that he had visited a great many of the places where these Permian breccias occur, and adds: "I scarcely ever found a large fragment *in situ*, and never one, large or small, with a polished and striated surface, . . . a proof that they are by no means common (Lyell's *Principles*, i., p. 223). Later and more complete investigations have entirely altered the situation.

Prof. Lapworth, whose knowledge of the rocks and of the district is unrivalled, declared emphatically in 1893 that every kind of rock found in the breccias had its exact counterpart in rocks *in situ* in the immediate vicinity and that the supposed Welsh rocks could be found "within a pistol shot" (*Glacialist's Magazine*, i., p. 80).

Prof. T. G. Bonney, F.R.S., has recently stated his opinion that the markings on these Shropshire breccias are not evidence of glacial action, and at the Nottingham meeting of the British Association he has further said of them: "I am convinced, after careful study of the specimens in the Museum of the Geological Survey, that they are due to earth movements and not to glacial action" (*Ice-Work, Present and Past*, p. 263).

Prof. Hughes refers to the specimens of the stones from these breccias preserved in the Jermyn Street Museum. These, he says, may be divided into two kinds, one represented by a single stone of a greenish colour and flattened oval form and covered with striæ undistinguishable from those seen on

stones from the true glacial drift. "This stone," he says, "must have been placed there for comparison and illustration, and having lost its label has got mixed up at last with those actually found in the Haffield conglomerate. I am well acquainted with that rock, and have no hesitation in saying that the stone I refer to was never in it. The others are specimens with obscure striæ, such as would be produced by movements in the rock," and he pertinently asks "why should we be expected to take on trust as evidence of glacial action in ancient times a few isolated fragments with the 'ghosts of scratches' on them when good observers, visiting the same spot afterwards, cannot find a trace?" (*Camb. Phil. Soc.*, 1893, pp. 111, 112).

In 1894 Mr. Oldham read a paper before the Geological Society of London on these same Permian breccias of the Midlands and the Carboniferous breccias of India and Australia. In regard to the first of these, to which, as we have seen, a glacial origin had been assigned, Mr. Oldham remarks that the first of Ramsay's reasons had been displaced by the proof that the stones in the breccias were local and not distant. Oldham goes on to describe the breccias as composed of more or less, but never very much, water-worn and subangular fragments of various sizes, the deposit being rudely stratified, the various beds shading off into each other. On Apperley Hill a bed of red-sand is intercalated, and the materials bear evident traces of having been rearranged by running water, but none of them have been deposited in a tranquil sea. Oldham concludes his examination of these breccias by rejecting any ice portage for them. He compares the beds to the great gravel fans that are found everywhere along the foot of the hill ranges of the drier part of Western and Central Asia. "They form a continuous fringe along the foot of the hills, often extending many miles over the plains. At their upper end the latter are mostly composed of large fragments, the interstices being filled with small gravel and sand, but further from the hills the larger fragments are for the most part left behind and the general texture of the deposit is finer. The pebbles, even to the outermost limit, generally remain imperfectly rounded, for when the streams flow after rain they are generally so loaded with débris as to be rather of the nature of fluid mud than water, and in

this the fragments of rock seem to be carried along *en masse* without being worn against each other to the same extent as in a mountain torrent. The stream, in fact, flowing over a surface of its own formation has developed such a slope and shape of bed that it is only able to transport its burden, and has little or no surplus energy to devote to the rounding of the rock fragments. Another effect of the large proportion of mud and stones moved by the streams is that occasional large blocks travel in the moving mass far beyond where most of their fellows have been left behind; occasional exceptional floods, too, may bring down larger blocks than usual, which afterwards get covered up by, and embedded in, gravel of much smaller grain.

"To such a cause," says Mr. Oldham, "I would ascribe the deposition of the Permian breccias—that is to say, they were formed by the action of streams subaerially and in the immediate proximity of the uplands which these streams drained. The deposit is too well bedded and the fragments are often too much waterworn to be an actual talus or a land-slip formation, extensive as these may sometimes be; it is not sufficiently regularly bedded to be a marine deposit formed in a tranquil sea, into which fragments of rock were dropped by floating ice; it does not exhibit any of those disturbances of bedding found in the marine clays of pleistocene age; and the very local character of the rudely defined strata, the way they pass into each other, combined with the general regularity and parallelism of bedding, seem to preclude the idea that they could have been deposited in a turbulent sea without the agency of floating ice or formed by a *débâcle*. There remains but the supposition that they were formed subaerially, like the analogous deposits of recent gravel fans, and in this case the angularity or at most imperfect rounding of the fragments precludes their being of any but local origin" (*Quart. Journ. Geol. Soc.*, 1894, pp. 464, 465; see also Beete Jukes, "South Stafford Coalfield," *Mems. Geol. Surv.*, 1859, p. 9).

In regard to the striæ on many of the stones, Mr. Oldham leaves the question of their origin in a doubtful way after urging that they were formed before deposition. His words are: "Not a few of the fragments I have seen are such as would unhesitatingly be accepted as truly glacial if they were

found in a recent moraine or in a pleistocene boulder clay; many are of more doubtful character, but not more so than specimens that might be collected out of a boulder clay of indisputably glacial origin; while it would be impossible to collect a specimen even from one of these last-named deposits for which, taken as an individual specimen and apart from its surroundings, it would not be feasible to suggest some other plausible explanation of origin" (*ibid.*, pp. 467, 468).

Turning from the very unsatisfactory evidence from Shropshire to that from temperate America, Mr. F. Bain, in *Science*, 10th March, 1893, describes the Permian rocks of Prince Edward Island. He mentions the occurrence at Carleton of a bed, regarding which he says "that the series of deposits appear to have closed in an important Glacial period, for on its summit rest not only drift fragments which must have come from the distant hills of New Brunswick, but a well-marked glacial moraine, now consolidated into a firm mass of conglomerate, five hundred yards in length, occurring in the bed of the mill river and reposing on the summit of the Permian and underlying the horizontal Trias" (*The Glacialist's Magazine*, i., p. 60).

Writing subsequently to *Science*, Mr. Bain says: "In 1887 I published in the *Canadian Record of Science* an account of a Permian glacial moraine in Prince Edward Island. I have recently examined the formation more carefully, and am not at all positive about its age. The bedding and jointage are conformable with the underlying formation, but the cementing material is purely calcareous, and the induration, though complete, may be recent. In the absence of organic evidence, I do not think we can positively say that this conglomerate is not Quaternary" (*ibid.*, p. 80).

This exhausts the cases in the temperate latitudes where supposed traces of glacial action in Permian times are said to have been found. If we turn elsewhere and examine the Permian beds in warmer latitudes and nearer the Equator we shall have a different story to tell.

We will begin with India. The case of the Indian Permian beds occupied me at considerable length in my former work (*Glacial Nightmare*, p. 288, etc.), and I am now only supplementing what I then said.

Oldham sharply separates the Permian breccias of the English Midlands from those of India. He says there is not the slightest real resemblance between the two. The English beds consist of a mass of fragments of stone of all sizes, mixed together and resting in contact with each other; the Indian and Australian beds, on the other hand, when typically developed, have always a tolerably, often an extremely, fine-grained matrix, itself distinctly stratified or inter-stratified with well-bedded rock. Through this are scattered in abundance blocks of rock of all sizes, but always embedded in and well separated from each other by the matrix. The matrix, he concludes, was deposited in quiet water, into which the fragments were dropped from above. Some of them are smoothed and striated boulders, while in two localities in India the underlying rock has a smoothed, striated and *roche moutonnée* character (*Quart. Journ. Geol. Soc.*, 1894, p. 468).

The Indian beds are known as the Gondwana series, from the old name of the districts south of the Nerbudda. This Gondwana series seem to me to be very illogically constituted out of beds belonging to two entirely different horizons, between which there is no community of contents. The lower portion, the speckled sandstones of Mr. Wynne, are correlated with the Carboniferous beds or perhaps Permian beds of Europe (a homotaxial inference which may or may not be subsequently justified), while the upper and entirely distinct series form the Carboniferous limestone of the same writer and the *productus* limestone of Dr. Waagen and the Indian Surveys. They have apparently nothing to do with any Carboniferous horizon at all, and are classed as Permian with certain Mesozoic elements by the Indian surveyors. At the base of the former or lower Gondwana series, as developed in the Salt Range, is a deposit of boulder clay. It contains a series of molluscs and other remains described on pages 120 and 121 of the *Manual of Indian Geology*, and showing them to be a marine deposit and not the remains of a glacier moraine. Elsewhere in India and its borders other conglomerates occur which are apparently to be dated as of either the lower or upper section of the Gondwanas, it being difficult in some cases to exactly class them from the absence of organic remains. In the Talchir beds in Orissa, however, they are associated with

beds containing certain well-marked species of ferns of the genera *Gangamopteris* and *Glossopteris*.

Let us first turn to the Salt Range beds (see *Glacial Nightmare*, pp. 290, 291). These Salt Range beds have continued to excite interest. They form a deposit very like a boulder clay, which lies unconformably on the older palæozoic beds and contains blocks of hard rock ranging to several cubic feet in size, almost always subangular and frequently showing faces that have been smoothed, polished and striated in the manner characteristic of glacier action. These fragments are principally composed of slates, quartzites and crystalline rocks. We are further told, and the fact is most remarkable, that their nearest analogues are to be found *in situ* not to the north, but 750 *miles to the south*, in the syenitic and porphyritic felsites of the Maláni series; but mixed with them are numerous fragments of the older palæozoic beds of the Salt Range, more especially of the magnesian sandstone. We are further told that many of the so-called glaciated pebbles of this bed show a peculiarity which is rare in other glacial boulder clays. Instead of being smoothed and striated on two surfaces only, they often have a number of flat surfaces meeting at other angles and each showing a different direction of striation (*Geology of India*, p. 120). They are in fact facetted. This seems to entirely differentiate them from true glacier stones. Nor, as the authors of the *Geology of India* admit, can we soberly understand how a gigantic ice-sheet could have existed in such a tropical latitude capable of carrying stones for 750 miles from south to north, and how its presence is consistent with the tropical mollusca in the beds, which also exclude icebergs (see *Geology of India*, second edition, pp. 120, 121).

Mr. Medlicott was the first to describe a similar set of beds in the valley of the Blaini, which perhaps belong to the upper Gondwana series. They have been traced over a wide district of the Central Himalayas from Mussooree at intervals to beyond Simla. They consist of a pure-grained matrix, containing numerous fragments of the volcanic rocks of the Jaunsar system when exposed at their outcrop in the Naira valley. They also contain rounded pebbles of quartz ranging up to a hen's egg in size, and in other cases angular and subangular frag-

ments of slate and quartzite up to some feet across in a matrix much resembling volcanic ash. The authors of *Indian Geology* say the agency by which blocks of stone were dropped over so large an area into a tranquil sea in whose bottom the matrix could alone have been deposited was no local one, and they attribute it to the action of icebergs in a tranquil sea. They very frankly say, however: "No smoothed and striated fragments have been found as yet, though an occasional one is met with showing striation like those produced by glaciers; but," they add, "the rock has invariably undergone much compression and disturbance, at times accompanied by distortion of the included fragments, which might account either for the obliteration of distinctly glaciated surfaces or for the production of those scratches which have been observed". Surely this is enough, and, notwithstanding the infrequence of other volcanic material in it, we may more safely identify the bed, as hinted at by Mr. Oldham, as a volcanic breccia (*vide ibid.*, p. 133).

A similar bed of conglomerate composed of subangular fragments and rounded pebbles of slates and quartzites is imbedded in a matrix of fine-grained slate. The rock, say the Indian surveyors, is in every way similar to the Blaini group of the Simla area, and the same arguments apply to it.

A third locality for these conglomerates is Orissa, the site of the well-known Talchir conglomerates, upon which I wrote in my former work (see *Glacial Nightmare*, p. 438), quoting the opinions of Blanford and Bauerman that they afford no evidence of the presence of glaciers.

Mr. Griesbach has lately made known from near Herat and the borders of Turkestan extensive boulder beds and conglomerates, lithologically resembling the Talchirs, underlying a great plant-bearing system with *Vertebraria*, a typical lower Gondwana plant, thus proving the northern extension of the Permo-Carboniferous beds of India.

Beds of similar age, as proved by their fossil contents (see *Indian Geology*, p. 121), occur in New South Wales, and at Bacchus Marsh, between Melbourne and Ballarat, in Victoria. These Bacchus Marsh beds contain large blocks of granite, subangular and ranging to several feet in diameter, some of which are conspicuously striated. There, as in India, they are con-

tained in a fine-grained matrix, containing delicate *Fenestellæ* and bivalve shells with their valves united, showing that the shells actually lived on the spot. Mr. Oldham argues that their deposition must have been by floating ice (*ibid.*, pp. 198, 199), a conclusion negatived, as I hold, by the character of the shells just named. Mr. Dunn speaks of this conglomerate as consisting of various granites, gneisses, schists, quartz-rocks, etc., ranging in size from fine grit to blocks several feet across, and weighing in some cases from twenty to thirty tons. Some of the stones are well-rounded, others are roughly angular and unabraded, while many are scored and scratched and great numbers are rubbed on one or more sides (Geikie, *Ice Age*, p. 821, quoting *Proceedings of the Australian Association for Adv. of Science*, 1890, p. 452).

Describing a similar conglomerate at Wild Duck Creek, the same geologist speaks of the indiscriminate manner in which the huge boulders, great angular and subangular masses, pebbles and fragments of rock of endless variety of size, form and material are commingled, sand and clayey matter binding the whole together. Many of the stones are of unusual forms and bear peculiar markings or modifications of their original shape; angular and subangular stones have been rubbed into shapes that water action would not produce. Boulders and pebbles evidently rounded by water have flat sides ground on them or bear marks not attributable to water action. Other masses have been planed, scored, striated, scratched and polished, and in many the edges have been broken away through others pressing heavily against them.

Planing is very common. It is sometimes flat, or in some cases the surface is hollowed, or there may be a convex surface. In either case the marks of grinding action are traceable in the hollow or on the convex portion as well as across the flat surface. In the case of the erratic called the Stranger (a mass of coarse-grained granite, measuring sixteen feet six inches in length by ten feet six inches in breadth at the widest part and five feet deep, and weighing about thirty tons) the present top surface must at one time have been wider, and it is planed for its whole length and breadth, both the felspar and quartz being cut down level. Mr. Dunn says the scratches are abundant on most of the stones. On the

intensely hard rocks like the quartzites fine scratches are noticeable with a glass; on soft pebbles very delicate scratches are observable, especially when freshly detached from the conglomerate, and are present in the hollows as well as the prominences of the stones. Beds of sandstone are intercalated in the sandstone, and thin strips of conglomerate bedded in the conglomerate. Many of the rocks forming the boulders in the conglomerates are foreign to Victoria, and although fragments of the bed rock of the country are preserved it does not follow that even they are of local origin. At one place the bed rock is exposed for a length of sixty feet, and shows a planed and striated surface, the parallel striæ running nearly north and south, the direction of movement being apparently from south to north. The critical fact to be noted is the heterogeneous mixture of huge blocks and rounded masses, smaller boulders, pebbles and angular fragments, sand and clay, all unsorted and indiscriminately commingled with the longer axes as frequently vertical as horizontal. Mr. Dunn attributes the beds partly to land ice and partly to icebergs (*Special Reports, Department of Mines, Victoria*, 1892).

Similar arguments for the glacial origin of the boulders at Bacchus Marsh have been advanced by Messrs. Officer and Balfour, namely, the unstratified nature of the clayey matrix, the number and variety of the included stones, the striated nature of many of them and their total want of arrangement. Mr. Officer was much struck by the resemblance of the conglomerate to true till. On removing some of it they found the bottom bed rock fluted and striated, the grooves and striæ indicating movement from the south. Mr. Brittlebant showed them a *roche moutonnée* he had uncovered presenting the appearance of three smooth parallel ridges well scored and striated with well-rounded grooves, six or more inches deep between, the grooves running north and south. Mr. Geikie quotes from a letter received from Mr. Officer in September, 1893, saying that in "Coimadai Creek, about seven miles from Bacchus Marsh, they found excellent sections showing most of the conglomerate *stratified* and lines of boulders in it as if dropped on a level surface. He mentions two fine boulders of granite both well scored, one ten feet long and the other eight; the latter had been broken up

and was originally seventeen feet long. These boulders occurred in bedded clay." The underlying Silurian rocks were grooved and striated, the striations being from south south-west to north north-east.

In the Werribee Gorge, six miles west of Bacchus Marsh, in Pyke's Creek, ten miles west of the gorge, and in the Lerderberg ranges, about eight miles north of the gorge, the Silurian surfaces are glaciated. "In one place, where the surface is inclined at an angle of 25° towards the south, it shows projecting bosses, all of which are finely grooved and striated. The *stoss seite* exposed towards the south is rounded off and highly polished where the rock is hard, and deeply scored in others." The striæ curve round the bosses as if they had deflected the ice. In other places large wedge-shaped slabs of the bed rock have been removed, the base of the wedges facing the south. Mr. Geikie says the *stoss seite* and *lee seite* can be well distinguished in the sketches sent to him.

At Korkuperrimal Creek the conglomerate is composed chiefly of waterworn stones with here and there a few striated ones, which seem to have been exposed to the action of water. In some of the conglomerates occur large blocks of granite, porphyry and a fine-grained greenish sandstone, all beautifully grooved and striated and not in the least waterworn. They everywhere repose on a grooved and striated rock surface (*The Great Ice Age*, p. 823).

In the *Manual of Indian Geology* we read that the New South Wales beds "contain large boulders of foreign rock exhibiting distinctly smoothings and striations, embedded in fine-grained silt, along with delicate *Fenestellæ* and bivalves whose valves are still united in the position in which they lived and died" (second edition, p. 198). In Australia these phenomena have occurred at two different horizons in the carboniferous measures: one in the Bacchus Marsh beds above mentioned, and the other in the Hawkesbury beds, which are at a higher horizon and separated from them by the Newcastle beds (see *Geology of India*, p. 198).

In regard to the latter I have already referred to them in my previous work. They have been described by Mr. C. S. Wilkinson in the *Journal of the Royal Society of New South Wales*, xiii., p. 105. They contain embedded in a matrix of

sand large angular fragments of shale in a confused manner, with their original bedding planes lying at all angles, and are accompanied by well-rounded quartz pebbles.

Mr. R. L. Jack has pointed out similar evidence in the Bowen River coalfield in Queensland, where large isolated erratics of granite occur in the midst of the sandy and argillaceous strata (*Report on the Bowen River Coalfield, Brisbane*, 1879).

Turning to Tasmania, we have a series of conglomerates marked by similar ferns to those which occur in the Talchir beds, and no doubt belonging to the same horizon.

Mr. Johnstone mentions his discovery in 1886 of huge erratics and polished blocks of the harder rocks foreign to the neighbourhood of this horizon at One Tree Point, Maria Island, etc., in Tasmania, since which time, he adds, he had found abundant evidence in rocks of the same age throughout south-eastern Tasmania. These erratics are for the most part found in barren layers of the *lower marine* beds, known as the mudstone rocks, but in certain places, as at Beltana, near Hobart, Bedlam Walls, Blackman's Bay Heads, Variety Bay, Adventure Bay, Esperance and Eagle Hawk Neck, the dropped erratics and conglomerates are associated in dense white or yellow close-grained sandstones with the following organisms: *Spirifera Darwinii, S. Strzelecki, S. convoluta, S. glaber, S. tasmaniensis, Terebratula sacculus, Sanguinolites Etheridgei, S. undatus, Pachydomus carinatus, Edmondia ovalis, Aviculo-Pecten limæformis, Tellinomya Etheridgei, Platyschisma occula, Orthonata compressa, Astartila cytherea, Theca lanceolata, Goniatites micromphalus, G. strictus,* also a fragment bearing a clear impression of the well-known fern *Gangamopteris spathulata.* The erratics are generally found in the middle or lower horizons, and are, when so, associated with the fine laminated zones almost wholly made up of the remains of the common lace-like *Fenestellæ.* There can, therefore, be no doubt as to the age of the rocks in which they occur. For the most part the polished or angular pebbles, boulders or angular blocks occur singly, embedded in what must have been at the time of deposition an exceedingly soft, fine, homogeneous mud, showing in these beds scarce a trace of lamination. In some places, however, they occur in thin lenticular patches, as an irregular conglomerate bed. The rocks are either

polished or angular; the pebbles or blocks grading from small pebbles to blocks over a ton in weight, principally composed of various kinds of granite, gneiss, quartzite, mica-schist, slate and quartz rock, all being rocks foreign to the locality in which they are found. The larger erratics, composed of granite or quartzite, generally occur singly.

The author refers to the difficulty of accounting for the huge single granite and quartzite blocks found at Beltana, One Tree Point, Bruni, Bedlam Walls and Maria Island, weighing respectively from half a ton to over a ton. They could not have been transported to their present position in the original soft, muddy bottom of the ancient and comparatively shallow sea-floor, and he invokes the ready agency of floating ice. He candidly confesses, however, to the absence of striæ and groovings (Johnstone, *The Glacier Epoch of Australasia*, pp. 33-35). So much for Tasmania.

In a paper by Mr. C. C. Brittlebank, read at the meeting of the Australasian Association held in January, 1898, it was remarked that all the so-called Permian glacial beds in the Bacchus Marsh district are more or less stratified, in which respect they differ materially from the glacial till of northern Europe (*Nature*, lvii., p. 495).

This evidence is interesting as showing common conditions occurring at the same horizon in India and Australia, but the case may be carried further, namely, to South Africa, where Mr. Blanford, Dr. Feistmantel, Mr. Oldham and others have recognised the Ecca and Dwyka strata as the equivalents of these Australian and Indian beds both by their animal and vegetable contents. Not only are the débris of life in these beds identical with the Indian ones, but they comprise a boulder bed composed of stones of various sizes embedded in a fine-grained matrix just like that of the Talchirs (*Geology of India*, second edition, pp. 202, 203).

I referred to some of these beds as described by Dr. Sutherland in my former work (*Glacial Nightmare*, pp. 297, 298). They are further referred to by Mr. C. L. Griesbach in his paper on the geology of Natal. He mentions the dark shaly rock with large boulders of older rocks imbedded in it of granite, gneiss, slate and frequently of greenstone; the boulders, often of a very large size, are embedded in a soft grit and shaly clay

containing small particles of mica. "The boulders," he says, "have not travelled very far, as many of them have kept their angular shape, and seem to have undergone a process of decomposition rather than of rolling." He adds that other writers on this bed had mistaken it for an igneous trappean rock, and he identifies it with the claystone porphyry of Mr. Bain. Referring to the grooves and furrows which Dr. Sutherland had mentioned as occurring on the plateaux of the Table Mountain sandstone, he says that quite similar grooves occur in great abundance on the sandstone of the Ifrimi river (*Quart. Journ. Geol. Soc.*, xxvii., pp. 59, 60). Similar beds occurring in the Orange River State were again studied by Stow, and his description of them is contained in his unpublished papers now deposited in the Natal Museum. Professors Ramsay and Rupert Jones have partially abstracted these papers.

Stow describes the conglomerates in question as immediately underlying the olive shales of the Karoo series, the earliest being thin lenticular patches alternating with the boulders.

In 1889 Stappf published a paper on what had been written up to that time on the Dwyka conglomerate of South Africa (*Das Glaziale Dwyka Konglomerat Süd Afrikas; Naturwiss. Woch.*, 1899), and concluded that the evidence for a glacial origin was not sufficient. Among other things, he remarked that the scratched surfaces could be explained in other ways than by glacial agency.

The South African beds on this horizon have recently again attracted attention. Dr. Molengraff, in a paper on the glacial origin of the Dwyka conglomerate (*Trans. Geol. Soc. South Africa*, 1898, iv., p. 103), concludes that the phenomena are only to be explained by supposing that land ice traversed the district—thus showing how greatly doctors disagree.

More lately, Messrs. Rogers and Schwarz, in an interesting paper on the Orange River ground moraine, published in the *Transactions of the Philosophical Society of South Africa*, xi., p. 113, etc., have described the phenomena as presented at Prieska and Hope Town. The conglomerate in which the so-called glacial feature occurs, say these authors, passes under the Kimberley shales or the lowest sheets of dolerite at the bottom of the shales. It consists of a mudstone "contain-

ing numerous small fragments of various rocks and minerals, but is on the whole a fine-grained hard mud with pebbles and boulders of various sizes up to two and a half feet in diameter embedded in it. The inclusions are scattered through the rock without any definite arrangement in beds, which show no signs of bedding. The pebbles are angular, sub-angular and rounded. *The edges of the large boulders are always more or less rounded.* A large proportion of the pebbles and boulders are scratched and flattened on one or more sides . . . and bear the closest resemblance to boulders found in the glacial tills and moraines which are forming at the present day. The principal rocks out of which the pebbles have been formed are quartzites and hard jaspery rocks, but granite, gneiss, amygdaloidal melaphyre, diabase and dolomitic limestone are also abundantly represented. These all exist *in situ* in the Prieska district west of the Doorn Bergen. They also occupy large spaces north of the Orange River as Stow showed. They do not include any fragments of the post-Karoo dolerite. . . . The presence of the conglomerate is always indicated by the numerous weathered-out boulders scattered over the veldt, often so abundantly that one cannot put a foot down without touching one or more of them. Many of these detached boulders show faint striæ on their flat sides, and some of them must weigh several hundred pounds." About thirty miles south of these beds our authors refer to other instances where the boulders are enclosed in a shaly matrix which is distinctly bedded "as if they had been dropped into their positions by ice floating in water covering a muddy bottom ".

Our authors calculate the maximum thickness of the conglomerate at some hundreds of feet. They describe, in addition to the boulders, beds of quartzite underneath the conglomerate as smooth, rounded and covered with scratches, the individual striæ being two feet long and covering each other at low angles with a general bend north north-east to south south-west. The northern sides of the polished mounds are smoothed, while their southern faces are rough and unscratched. One such *roche moutonnée* rises about ten feet from the veldt, and its long north slope is about sixty feet in length. A very good photograph is given of this feature. We are further told that isolated boulders of rocks, foreign

to the locality, are found scattered over the quartzite hills, and, according to our authors, prove that the whole area was once covered with conglomerate ! ! ! In the case of an upright wall of rock at Jackal's Water the face of rock is polished and scratched, and these features pass round the edge of the rock. "It is particularly noticeable that the lower parts of the hills alone show the striations. The upper portion have the very rough surface characteristic of the hard rocks of this district when exposed to great variations of temperature and unprotected by vegetation."

At Vilets Kuil, in the division of Hope Town, again, a mass of felsites and breccias rises above the general level in the slope of low hills. The surface of these beds also is hummocky, and the northern slopes of the hummocks are smoothed and striated, while the southern are much steeper and rough. Here, again, only the lower portion of the felsite, where it emerges from the conglomerate, has these marks. It is unusual to find the striæ preserved at a greater distance than 200 feet from the conglomerate. Our authors remark that it is only the quartzite and compact felsite that bear these marks; the granite, gneiss, melaphyre and crystalline limestone do not show them, but the inlayers of granite have the form of *roches moutonnées*. I ought to add that this paper is illustrated by some excellent photographs.

The facts here mentioned are very interesting and curious. As in India, only on the other side of the Line, the drift of the boulders is from the equator towards the poles, which is a curiously inconsistent fact if the distribution of climate in Permian times was in any way as it is now. The fact of the beds being in so many of the localities stratified, also shows that they were laid down under water, and precludes the notion of an ice-sheet.

Let us move on, however. Recently evidence has been forthcoming that beds at the same horizon and marked by the same fossil plants occur also in South America, and that similar beds occur especially in Argentina, containing Gondwana plants, as shown in a paper by Dr. Kurtz, translated in the *Records of the Geographical Survey of India* (*Nature*, 17th October, 1895).

In a letter from Orville A. Derley to Dr. Waagen, on a

Carboniferous period in South America, published in the *Neues Jahrbuch*, 1888, vol. ii., pp. 172-176, he tells us how he had found in the province of Parana rounded fragments ranging in size from a man's fist to four times the size of a man's head buried in an extremely fine clay shale; also, in the province of Itú, on the river Juli, in Itapetininga, and many other localities, in an extraordinarily fine-grained sandy shale, isolated round blocks, a foot and a half or more in diameter, of granite, gneiss, etc. On the Capavery river, boulders over a metre in diameter were discovered resting in a matrix of carboniferous clay shale. No striations were observed (*American Geologist*, iii., p. 320).

Mr. J. Geikie does not disguise from himself the difficulty of accounting for a glacial period in Central India, and only 20° from the equator, while in Africa the similar phenomena are only 30° from it. He speaks of it as presenting a very hard nut to crack, since it would point to the existence of a more extensive and severe glaciation than that of pleistocene times. But he hardly lays enough stress on the actual difficulty. The case is put more pointedly by Mr. Oldham. He says "the remains of this carboniferous glacial period, so conspicuous in India, Africa and Australia, all lie within, or only just beyond, 30° from the equator. The furthest from the equator lie in latitudes where the last glacial period of pleistocene times has left but few traces at low altitudes, and those of a somewhat doubtful character, while most of the remains are in latitudes to which the ice of the pleistocene glacial period never penetrated, and many are well within the tropics. Not only so, but the corresponding deposits, not merely of the temperate zone in Europe and America, but even within the Arctic circle, in which one would expect the traces of this cold period to be more abundant, more extensive and more conspicuous, are almost free from traces of glacial action." He then speaks of the very sporadic character of the boulders from English carboniferous rocks as if they lay in a latitude which was near the limit of floating bergs. It is not merely the fact that the phenomena quoted from the Permian beds occur in low latitudes, while they are apparently absent from high ones, which makes them striking, but the further fact that they apparently occur in a belt about the equatorial region and not in

the temperate and Arctic regions. The arrangement of these beds in an equatorial belt puts out of court all explanations of their being dependent upon astronomical changes of climate.

Mr. Oldham, who presses this view, is nevertheless embarrassed, because he is an adherent of the glacial theory, although a very moderate one. He therefore falls back on the old notion of a change in the position of the earth's axis. This is, we have shown, absolutely inadmissible on physical grounds; but if it were otherwise admissible, how we are to explain the Permo-Carboniferous beds and the equatorial belt they form as the result of the culmination of terrestrial cold at two fresh poles of the earth instead of the present ones I cannot understand. The difficulty would be just as great as it is now, wherever we put the poles.

No shifting of the earth's axis, nor any other physical cause known to me, nor any unknown to me, which did not steep the whole world in snow and frost and strip it of every form of life which is not glacial would cause an ice age to occur about the earth's waist in this fashion; and if it did so, the contemporary beds in higher latitudes ought to show traces of it—which they do not. If we discard the notion of a general ice age and fall back upon local glaciers, we shall not, so far as I can see, be any better off. Local glaciers cannot explain the phenomena in India, where the stones have travelled over 700 miles; nor will they explain the phenomena anywhere, since the stones, so far as we know, in these various cases have not been distributed sporadically, but in a definite direction.

Again, in order to have such glaciers in the very tropics we should require very big mountains, like Mount Kenya or Kilimanjaro, or we should not get any permanent snow to feed them. But how are high mountains to explain beds whose stratification in many places shows them to have been laid down under water; and how, by any process, are we to reconcile ice in the form of either ice-sheets or glaciers or icebergs (which last would need great ice nurseries on the land to feed them) with the mollusca and plants in these beds?

It seems to me, therefore, as plain as can be that the Permo-carboniferous beds do not testify to the existence of an ice age when they were deposited. It seems, in fact, plain that we must in the case of these beds turn to some other cause

than ice to explain their idiosyncrasies. The fact is that their real lesson has been misread. They not only teach us that certain phenomena which have been supposed to mark a glacial age in Permo-Carboniferous times do not do so, but that similar phenomena in Pleistocene times do not need the monstrous postulate of an ice age to explain them either. I hold that these beds, instead of testifying to the existence of an ice age in far-off Permian times, testify in fact the other way, and show that we have been pursuing a fantastic cause when we have appealed to ice at all to explain the Drift and similar beds at a later horizon. What explains one phenomenon will probably explain the other; and what seems plain is that if an ice age is unavailable in the one case it raises a strong *a priori* presumption against its being available in the other.

Let us now consider a third and cognate class of evidence, which seems to create still another *a priori* case against the glacial theory, namely, the existence in the tropics of a series of facts dating not from remote Permian times but from the very latest geological age, if not actually resulting from current causes, which, like some of the Permian phenomena, simulate very closely the features of the Drift, but which we cannot, without doing a grave injustice to all probability, suppose to have ice for their originator, since they occur in the torrid latitudes of the world. I have referred to these effects generally in my former work, and argued that they are not to be accepted as evidence of a general tropical glaciation as Agassiz and Belt would have maintained if they had been here, and as I believe my friend Mr. Blanford still maintains in spite of the opposition of the great mass of geologists. I am now referring to the same facts for another purpose altogether, namely, as proving that if these phenomena are to be explained not as the result of ice action but of something else, it is reasonable and rational that we should explain the drift beds of our latitudes by the same cause.

In my former work I condensed the account given by Mr. Belt of his experiences in Nicaragua, where he claims to have found unstratified drift, 200 feet in depth, consisting of quartz sand enclosing angular and subangular boulders of quartz, etc. Many of these boulders are very large, some fifteen

feet across, and some have travelled quite eight miles from their original site. He mentions a ridge 1,200 feet high composed entirely of boulder clay, full of angular and sub-angular blocks of stone of various sizes up to nine feet in diameter. "This boulder clay," he says, "extends all the way from San Rafael, and ranges of hills appeared to be composed entirely of it." He noticed rounded and smoothed masses of rock, moraine-like accumulations of unstratified sand and gravel and far-transported boulders. He noticed no scratches, but this he attributes to the weathering of the rocks (*The Naturalist in Nicaragua, passim*, and *Glacial Nightmare*, etc., pp. 274-278).

Belt's account of the Nicaraguan phenomena was confirmed in 1891 by J. Crawford in a paper published in the eighth volume of the *American Geologist*, p. 306. He says that none of the mountain ranges in Nicaragua rise to the altitude of the frost line, and only four have peaks whose highest points are 6,500 feet above the Pacific. The average height of the Cerros in that country is about 2,800 feet.

Crawford describes how in a valley, through which flows the Chomeha river, he found many *moutonnéed* masses of granite and striated and polished masses and large flat loose rocks striated from one and a half to two inches deep in lines parallel with the flow of the creek. Near the termination of the cañon, and extending across the valley to the Rio Seibeta, are several mounds and hills composed, so far as examined, of unstratified deposits of clay and sands containing rough and smooth-edged, large and small rocks and pebbles indiscriminately mingled. From each hill there projects, in the direction of the Rio Seibeta, a ridge apparently composed of the same materials as the hills. These, says Crawford, would undoubtedly be designated "moraines" in Minnesota, New York or Canada.

Of another cañon valley, called the Tungla, he says: "Smooth oval-surfaced areas and deeply-striated masses of rock are frequently visible in and near the bed of the small stream flowing through the valley. Only a few scattered hills and ridges, but partially examined, were composed of unassorted deposits of boulders, rock fragments, sand and clay, but these, in some parts of the ridges, were cemented together.

"On the Pacific side of the Cordillera, in Nicaragua, the mountains terminate in a large *mesa* named Totumbla, across whose summit runs a shallow valley about two miles wide, on which are exposed at several places large masses of rock having smooth rounded surfaces and measuring 50 to 200 feet long. Some of these masses are polished. Near the edge of this valley are numerous flat, striated boulders and loose striated rocks of local origin. The most numerous striæ are parallel with the general direction of the valley. This *mesa* is 3,260 feet above the sea level. . . . At the lower end of the cañon, which runs into the Rio Veijo, are numerous hills and knolls, many of them having long connected ridges that extend far into the valley. Those examined, and probably all of them, are composed of irregularly mixed unstratified rocks, clays, pebbles and sands, cemented in some places by iron oxides and elsewhere but partly hardened."

Crawford then goes on to refer to the similar phenomena previously described by Belt and occurring between Ocotal and Depilto, and consisting, as he says, of such apparent evidences of glaciers' work as striated boulders, deposits of sands, clays, pebbles and rocks, generally appearing to be unassorted. But in some parts, he adds, they appear stratified, not as deposits made by recent floods, having well-defined lines of separation, but as if made uninterruptedly and at the same time with the other large masses of (so far as examined) unassorted deposits. He concludes, after several examinations extending over four months, that these deposits are moraines. Several large knolls and hills north of the village of San Rafael del Norte, each from 150 to 600 yards long, declining into the Rio San Rafael del Norte, were composed, as far as his observation extended, of the unassorted, unstratified fragments of rocks and pebbles; some rough, others smooth-edged; all embedded in sands and clays. In some places the sands and in other the clays were in excess. These also were described by Belt as moraines.

Crawford confesses that the watershed of the three rapid rivers Segovia, Depilto and Maculeso is quite large, and in flood time they transport large boulders for long distances, that there are evidences that a part, if not all, of the valley

has been overflowed, and that he found many loose boulders on the side of the mountain of Cerro Yalli, too many, as he thought, to have remained in such places after a glacier had moved over them.

The only trace of a colder climate, which was attested by the biological evidence, was the presence of *Pinus sylvestris* and other cold climate conifers (dwarfish) on some of the highest mountains of northern Nicaragua. In order to explain the glaciers, Crawford argues that at the time a Nicaraguensian continent extended eastwards about 1,500 miles farther than at present over the north and middle part of the area now occupied by that part of the Caribbean Sea east from the Nicaragua coast to the Atlantic Ocean at about the sixtieth degree meridian west of Greenwich. He also refers to the evidences of upheaval and subsidence in pleistocene times furnished by the features of the country.

These facts have been a great stumbling-block to many people, and not least to the champions of the glacial nightmare. To suppose that great ice-sheets existed in the hottest parts of the new continent, and came down to within 2,000 feet of the present sea level, when we remember the very tropical vegetation and the very tropical animal life existing in this district, and which must have lived through such conditions, seems quite incredible. The first impulse, and one to which I myself gave way, was to attribute the phenomena described by Belt to enormous local glaciers; but this does not commend itself to me now. It seems very plain that such results as are described by Belt are unmatched by anything in the shape of glaciers known to us, and are especially incompatible with glaciers in low latitudes; and even if they were so compatible, they would involve the postulate of such vast mountains existing in Central America at this time, in spite of much evidence to the contrary, that they would appear to be entirely out of court. That the phenomena in question are closely related in kind and degree to those of the Drift of higher latitudes I am quite disposed now to admit. But from this I draw the conclusion, not that there was an ice age in Central America, but that the facts presented equally by the Nicaraguan deposits and those of the Drift must be explained by some other cause than that of ice. The

very fact of ice on a vast scale being an impossible postulate in Central America raises a very strong presumption, in fact, that it was some other cause which produced the effects so persistently attributed to ice elsewhere.

Nicaragua is not the only part of the tropical New World where phenomena of a similarly ambiguous kind have been observed. Long ago, as I mentioned in my former work, Schomburgk called attention to the presence of very large erratics in an even hotter and more tropical district, namely, British Guiana.

That writer, in his account of British Guiana, published in 1840, speaks of a peculiar feature of the country being large tracts of boulders, mostly of granite, more or less accumulated in particular places, sometimes with great confusion, and which, wherever they traverse the rivers, form rapids and cataracts. They assume their grandest features in the tract which is called Achra Monera, on the river Essequibo, in latitude 4° 20′. Further on he speaks of the Comuti or Toquian, consisting of a huge boulder of granite, surpassing in size the celebrated pedestal of the statue of Peter I. On this rests an oval piece of granite, which bears a third in the shape of a jar; it is covered by a fourth (*op. cit.*, pp. 8-10).

The same writer (*Trans. Geog. Soc.*, 1841, vol. x.) describes how he saw these great masses of transported boulders lying on the sedimentary beds in a region, says Murchison, on which no glaciers could ever have existed. Schomburgk tells us he passed near Pukasanti numerous *erratic* blocks of gneiss contorted in a very remarkable manner and with large fragments of quartz embedded in them. One of these blocks was large enough for some hundreds of men to have encamped on it (*op. cit.*, p. 162). In travelling across several streamlets flowing into the Guidaru, he says: "At times we came upon large tracts or bands 200 yards wide, in a west by north direction, of angular pieces of quartz, as regularly placed as if laid for paving; at other times we crossed a track of granite boulders laid in the same direction, and at a distance resembling fortifications. Near one of the most irregular of these *erratic* blocks, named Scai, the last cacique of the Caribs, the celebrated Mahanarva, had lived. The settlement of Water Tecaba is placed in the midst of granite boulders" (*ibid.*, p.

166). Further on he speaks of passing numerous blocks of greenstone, etc.

If the facts in Nicaragua are difficult to explain by ice, how are we to explain these mentioned by Schomburgk within five degrees of the equator on low savannahs, and with only low hills within any accessible distance? Ice is as likely to have transported the great stones in this locality as it is to have smoothed the sandy wastes of the fiery deserts of Arabia and Africa. Phenomena of a cognate kind have been reported from Jamaica, which I described in my former work (*Glacial Nightmare*, p. 499).

Let us now turn from the New World to the Old, and first in regard to Africa. As long ago as 1866 Signor E. Lombardini published a paper on "Traces of the Glacial Period in Central Africa" (*Rend. dell' J. R. Ist. Lombardo di Scienze*, etc., 1866, vol. iii., and *Giorn. dell' Arch. ed Agron xiv.*, vol. xiv.). In the former paper Lombardini quotes the *Saggeo Idrolico nel Nilo* and its two appendices (p. 85). I have read the paper, and the whole conclusion seems based on the postulate that the Central African lakes, whence the Nile springs, were formerly much larger, and that the great depression which exists in Central Africa points to much greater erosion, and therefore to much greater evaporation, and therefore to a glacial epoch in Central Africa in old days. This argument leading up to these conclusions of the paper seems to me quite absurd—but then I am not a glacial champion.

Farther north, but still under a latitude less than about 35° north, Mr. Shaw described great boulder beds as existing partly on the flanks and partly in the upper valleys of the Atlas. The two sets of phenomena seem to differ in kind and origin. "The former began," he says, "to appear in the form of scattered blocks of red sandstone, remarkable for their large average size, many of them being ten or twenty cubic yards in contents." "At the mouth of one of the valleys," continues Mr. Shaw, "we suddenly came upon a huge development of these red sandstone boulder beds as great ridge-like and very symmetrical masses, with terminal faces 300 or 400 feet high, and intermixed with but a very small proportion of fine matter." Following the escarpment which bounds the valley, he says, "below it the great boulder beds still occur in great masses, not resting directly

against the escarpment, but as isolated mounds 200 or 300 feet in advance, sloping down towards the escarpment in one direction and in the other rolling away in great wave-like ridges and undulating sheets, which terminate at a well-marked line of demarcation, just where the level portion of the plain commences. I measured by aneroid the height of these mounds, and at one point the summit was 3,950 feet above the sea level, from which they spread down uninterruptedly to the edge of the plain, nearly 2,000 feet below. . . . They look like a row of pyramidal tali resting against the face of the escarpment, as if they had been cast down from its edge on to the plain. In internal structure the boulders are arranged in layers, sloping away from the escarpment towards the plain, *and on a nearer view it is seen that the individual mounds are not connected with channels or valleys breaking through the escarpment.* . . . The depression between the escarpment and the drift mounds is a remarkable feature, and suggests an entire change of conditions since the boulder beds were deposited."

What resemblance there is here to glacier phenomena I do not know; and it requires some courage to postulate the existence of glaciers which did not deposit moraines of heterogeneous "muck" (to use the American phrase), but only clean blocks of sandstone, etc., at an elevation of only 2,000 and 3,000 feet, where there is now no perpetual snow at all.

Higher up the Atlas, and at the heads of the valleys, Shaw speaks of unquestionable moraines commencing at an altitude of 6,000 feet at the village of Eitmasan, in the province of Reria, where he met with a gigantic ridge of porphyry blocks having a terminal angle of repose of between 800 and 900 feet in vertical height and grouped with several other mounds and ridges of similar scale all composed of great masses of rock with little or no admixture of small fragments and completely damming up the steep ravine and retaining behind it a small alluvial plain 6,700 feet above the sea level. "We failed," he says, "to detect *any scratched blocks or striæ*, but that these ridges are true glacial moraines no one who has seen them and compared them with other glacial phenomena would for a moment doubt" ! ! !

Similar beds to those on the north flank of the Atlas range are also described from the south of the range by M. Grad.

He says he found them at the entrance to the gorge of El Kantara, but he noticed no moraines or striated stones. Similar deposits, which he calls moraines, were seen by M. Maupas near Blidah, which Grad mentions, and he also says that Leblanc had noticed erratic blocks in the same district in the *Bulletin* for 1838 and 1839. In the discussion which followed the reading of M. Grad's paper, M. Mares said "*que les amas de matériaux de transport signalés aux environs de Blidah lui paraissent des cônes de déjection de torrents, notamment de l'Oued el Kébir; il ne peut y voir des moraines*" (*Bull. Geol. Soc. of France*, 1873, p. 87). This is a good test of the value of the other testimony about the occurrence of so-called glacial effects in the Atlas. The effects are no doubt there, but to call in ice to explain them under the conditions named seems merely fantastic. I shall revert to this and other cases of supposed glacial action in low latitudes presently.

Mr. Blanford, in describing the vast beds of loose deposits which cover so much of the land in Persia, says the deposit must be very great since hills of solid rock but rarely emerge from it. The greater part of it on the slopes consists of sand or pebbles, the latter more or less angular and mixed with large blocks, all derived from the adjacent hills. Fragments two or three feet in diameter are not uncommon even at a distance of a mile or two from the base of the hills, but he says he only noticed them where small streams issue from the higher ranges. At higher elevations there is a covering of gravel and clay with boulders. In describing these beds Mr. Blanford makes the significant comment that although large blocks, some as much as two feet in diameter, were common in the higher deposits, he could find no such angular fragments as usually characterise a boulder clay, all the pebbles and boulders being more or less rounded. In most cases the deposits were destitute of stratification, but in places they were distinctly bedded, and in one instance, not far from Karmán, have been much disturbed (*Quart. Journ. Geol. Soc.*, xxix., p. 493, etc.).

In regard to Peninsular India the evidence seems plain. In the *Manual of Indian Geology* we read of that district: "There is no physical evidence, so far as is known, of a geologically recent cold epoch, and some geologists have doubted

whether India was affected by the glacial period" (*op. cit.*, p. 14).

Again, speaking of the indications of a colder period in the Peninsula, the writers of that work say "they do not point to a sufficient diminution of temperature of the Himalayas to make it probable or even possible that there should be any actual physical proofs of the glacial period having been felt in the peninsula" (*ibid.*).

In regard to the Himalayas, on the other hand, the same writers (one of whom by the way is a strong adherent of the glacial nightmare in its extreme form) tell us that there is everywhere abundant evidence of the glaciers having extended to lower levels than they reach now. And again we read: "The positive and unmistakable proofs of a period colder than the present are sufficient to enable us to discard all the more doubtful evidence, and more recent investigations have shown that it cannot be attributed, as was once suggested, to a lower general elevation of the Himalayas than they now attain" (*ibid.*).

This brave statement, I am bound to say, is hardly supported by such evidence as one would have expected. On page 484 of the same work we read that "the Himalayan glaciers never spread over the low grounds in great ice-sheets like those of Europe"; and again, "in the Kangra valley there is good reason to suppose that the glaciers once reached to below 2,000 feet above the sea, and the erratics of the Potwar show that ice in large quantities was not unknown there at one time." These are the only facts forthcoming so far as I can see, and they are rather hypotheses than facts. There is, in addition, an *obiter dictum*, directed against myself, in which we are told that it is impossible for anyone who has studied the action of subaerial denudation not to see that the forms of the hills and their intervening valleys are due to the action of rain and rivers aided by frost; while we are told that a certain photograph, which forms the frontispiece to the volume, shows to the initiated eye that the shape of the mountain is due to the disintegration of the rock by frost and the removal of the débris from the hollows by streams and glaciers, and not to any disruptive force. In regard to mountains and valleys being the handiwork of frost, rain and rivers, I have said enough in the previous chapters. To me the notion is as

reasonable as that warm water and a sponge excavated them both. In regard to the "initiated eye," I take it to mean an eye properly initiated into the mysteries and vagaries of glacial geology—*i.e.*, a glacial eye, which I have not unfortunately got, nor have I been able to borrow any spectacles from those who always see ice everywhere, to suit my sight. That the corroded surface of the mountain may be due to erosion is probable; that the mountain was itself cut out and shaped by such means is to me impossible.

On the main question I prefer to quote another observer of great experience, who has an initiated eye. Mr. Griesbach says that "during years of observation he failed to find striated and polished boulders in the moraines of the Himalayan glaciers, while the bounding rock walls of the glacier valleys do not show the smooth and rounded surfaces usually seen in other regions" (*Geol. Mag.*, 1892, p. 269). That is enough for me.

The evidence from India, therefore, is not very hopeful for those who think the Ice King once put his arms round the waist of our mother Earth. One more case must suffice, and it is from a notable place, namely, the Azores. "At my request," says Darwin, "Sir C. Lyell wrote to Mr. Hastings to inquire whether he had observed erratic boulders on these islands, and he answered that he had found large fragments of granite and other rocks which do not occur in the Archipelago" (*Origin of Species*, p. 441). These erratics are surely a serious difficulty for those who claim such stones as the best evidence of ice portage. How can we invoke ice-sheets or glaciers in the Azores, or understand how icebergs could sail over those islands whose fauna and flora prove that they have been above the sea so long? Surely, like the granite blocks on the cliffs of South Australia, they testify to some other portage than that of ice; and, in doing so, they, like the other phenomena here quoted, strengthen the contention of those who attribute the phenomena of the Drift to some other cause than a glacial age, and they thus create another powerful *a priori* argument in their favour.

CHAPTER XI.

THE BIOLOGICAL EVIDENCE ABOUT THE SO-CALLED ICE AGE AND THE QUESTION OF INTERGLACIAL PERIODS.

"We might expect that as we come close upon living nature the characters of our old records would grow legible and clear; but just when we begin to enter on the history of the physical changes going on before our eyes, and in which we ourselves bear a part, our chronicle seems to fail us—a leaf has been torn out from nature's book, and the succession of events is almost hidden from our eyes."—Sedgwick.

WHEN Croll formulated the theory by which he made his great ice age extend over a whole cycle of years, from 240,000 B.C. to 80,000 B.C., he urged that such an age necessarily included several minor periods involving alternations of climate, and hence he introduced the notion of interglacial periods. The notion does not seem to have occurred to geologists before, but a considerable number of them now followed the lead of Croll, and interglacial periods became a favourite expression with them, and especially with the champions of a glacial age who belonged to the Geological Survey; and, what is rather curious, a notion which was originally started in order to support the astronomical theory of an ice age, and which would hardly have occurred to geological students unless pressed by the necessity of supporting that prime postulate of so many so-called uniformitarian geologists, has, with some writers, apparently survived the astronomical theory and is maintained for its own sake. In my *Glacial Nightmare* I enlarged at considerable length upon the subject of so-called interglacial deposits, and ventured to question the relevancy of the evidence adduced in their favour. My views on this matter are shared I know by some of the most experienced of living geologists. I shall now, as in the previous chapter, supplement what I

then said by such facts and arguments as have since reached me, and as the matter is an important one (the living champions of interglacial periods being a numerous and powerful phalanx) I shall enter into considerable detail in my criticism.

Dr. Croll says the glacial epoch may be considered as contemporaneous in both hemispheres, but the epoch consisted of a succession of cold and warm periods, the cold periods of one hemisphere coinciding with the warm periods of the other and *vice versa* (p. 234). This, as Prestwich says, would involve an indefinite succession of interglacial periods, but only one definite interglacial period during the glacial epoch is brought forward. Dr. Croll, however, accounts for this by the convenient argument that "the geological evidences of the cold periods remain in a remarkably perfect state, while the evidences of the warm periods have to a large extent disappeared" (p. 238). If, however, as Prestwich replies, one instance could be well preserved, might we not expect other instances to occur in some of the many localities affected? (*Op. cit.*, p. 6.)

We ought to remember, even if we admit the plea that denudation in glaciated areas must, *ex hypothesi*, have been enormous, that we cannot suppose the climatic variation was limited to the zone where the postulated ice-sheets occurred, but must have greatly affected the climate further to the south or north, where the pleistocene beds and their covering exist undisturbed and in full sequence. Thence, surely, such evidence ought to be forthcoming. But apart from this I cannot understand how ice-sheets could discriminate in the fashion supposed between beds containing the débris of cold epochs and those of warmer periods; surely the delicate *yoldias* and other arctic shells and the leaves of the polar willow are more and not less fragile than mammoths' teeth, etc. The fact of the matter is that Croll's contention in this behalf is almost childish, and is the offspring of a despairing logic.

The evidence which has been appealed to by the champions of interglacial periods is of two kinds: one is biological and the other stratigraphical. In the present chapter I shall deal with the biological evidence, which, it seems to me, has

been very largely misread, and which is being at last put into proper focus.

I will begin with the remains of land and fresh-water animals and plants which have been treated as evidence of interglacial climates, and I am brought at once face to face with a problem which has greatly exercised geologists for a long time, namely, whether the mammoth and the animals associated with him, including paleolithic man, were pre-glacial, post-glacial, or interglacial, or, as I prefer to state the question, whether they lived before, after or during the distribution of the drift beds. The view for which I have fought for many years is that they lived before this distribution and not after it. The glacial champions who admit that the fauna did live before their glacial period but that it lived also after it (whence one of them named the mammoth *bicyclotherium*), or who hold that the same fauna and flora recurred in successive mild periods separated by long intervals of cold and desolation, have, it seems to me, a difficult nut to crack in explaining the continuance through long ages with intermittent conditions of unchanged forms of life. D'Archiac long ago pointed out the extreme improbability, if not impossibility, of such a thing happening. I will here quote a few sentences from my own former work: "If we limit ourselves to much more recent times we shall find no support for the contention of the glacialists. In the bogs of Denmark and South Sweden we have a succession of beds, beginning with those containing stone axes. In these bogs we find the remains of a succession of forests, but not of the same trees. We begin with pines and birches, then we have oaks, and lastly beeches. The same thing is found in the bogs of Holland. Erman tells us that after a fire in Siberia which had burnt down a pine forest, there grew up, not pines, but birches and poplars, and the same observation has been made in America. If this be so where the conditions are continuous, and where the seeds of the old forest are scattered about, *a fortiori* would it be so when a glacial epoch intervened?" How strange would it be to find that, after an interval of ten thousand years of Greenland, Merry England should revert to almost precisely the same fauna and flora living in precisely the same counties, and coming back not

to the old rich soil, but to a soil made terribly barren and cold by the deposition on it of the so-called glacial clays and sands. Speaking on this point Prof. M'Gee says we know that crude soils, such as fresh glacial clays, from some yards beneath the surface are not adapted to the support of a luxuriant vegetation. Thistles, yuccas, cacti and other hardy plants may spread over barren clays, sterile sands and tufaceous wastes, and are sometimes planted with the object of reclaiming such areas, but even when other germs are not lacking it is only after considerable periods that a richer flora supersedes these plant pioneers. After the retreat of a glacier, however, there would be a dearth of seeds and germs, and their spread over the glaciated wastes would be slow (*Geol. Mag.*, New Series, vi., p. 420).

This seems perfectly sound, and it is an answer not only to those who champion interglacial climates, but to those even more irrational people who argue that in a period supposed to have been marked by ice-sheets covering a third of the northern hemisphere the retreat of the ice was closely followed up by the growth of luxuriant forests and a teeming life, and who compare the conditions of a world when the North Sea was frozen tight with those prevailing in the many Alpine valleys at the feet of some glaciers, or with the seaboard washed by the temperate waters of the North Pacific. The fact is that apart from all such critical difficulties there is a constant change and development in progress in the flora and the fauna of a country, and to suppose that it is possible to have a succession of intermittent biological periods marked by the same fauna and flora is contrary to all the evidence of paleontology.

Let us, however, turn from these *a priori* difficulties to the evidence itself. What, then, is the real horizon of the mammoth and its companions, including paleolithic man?

It is at once a reproach to geology, and a good proof of the very great difficulty there is in making our way among the latest geological records, that there should be any doubt upon such a question as the exact horizon of the mammoth and of paleolithic man. The fact is nevertheless so, and has given rise to a sharp polemic in quite recent years. I am bound to say that I myself have changed my views on the

subject in view of the evidence, and have to recant some phrases I printed long ago in the *Geological Magazine* and afterwards withdrew.

I wish to speak with some precision in the matter, and not to be misunderstood. The point I would discuss is not whether the mammoth lived before, during or after the so-called glacial period, but whether the beds in which his remains are found, when undisturbed, underlie or overlie the drift or are intercalated with it. The two questions are not, as is often assumed, the same, and it is to the latter alone that I wish my criticism at this point to apply.

I must also define the kind of evidence which I alone think conclusive. I altogether distrust any evidence on the point except that of superposition. Evidence based upon inferences of different kinds I have always mistrusted in deciding this critical question, but I would go further. We must remember that the till or boulder clay contains extraneous bodies of different kinds, which are often far-travelled and sometimes not so. Among these bodies there are tree trunks or molar teeth of elephants, which may be as true boulders as those of granite and gneiss, and, like them, derived from elsewhere. Whatever theory we adopt in regard to the till, we must concede that it picked up far-travelled débris in its march, and mixed it in many cases with the débris of the underlying beds, sometimes with chalk, sometimes with lias, sometimes with sandstone, and mixed this débris with the fossils from these different beds. It is further clear that ice or water would take up and convert mammoth teeth into boulders just as readily as it would ammonites and belemnites, and that the former would be no more contemporary with the till nor evidence of an interglacial climate than the latter are.

In regard to such organic boulders I find myself in complete agreement with Prof. James Geikie. "In the mass of the till fossils sometimes, but very rarely, occur. Tusks of the mammoth, reindeer antlers and fragments of oak and other trees have from time to time been discovered in this position. They almost invariably afford marks of having been subjected to the same action as the stones and boulders by which they are surrounded—that is to say, they are rubbed, ground, striated and smoothed. Sea-shells, broken, crushed and

striated, also occur under similar circumstances in certain deposits of till" (*Great Ice Age*, 3rd edit., pp. 104, 105). On a subsequent page Mr. Geikie argues that these may be interglacial rather than glacial, a conclusion I utterly fail to find any justification for. It seems to me plain that the animal and plant remains which are found at all horizons in the clay can only exist there as boulders, and that it is impossible to believe they were living during the deposition of the clay or that they index interglacial beds of which we have no other evidence.

If this is so with detached boulders and pieces of wood, it is equally so with occasional sheets and masses of peat and other intercalated masses, which are merely boulders of another kind. On this subject Mr. Geikie, again, has some very apposite remarks. He described how in some places tongues of boulder clay have been intruded or injected under pressure into the beds below, forming what workmen call "legs," and he quotes some notable instances at Kelsea Hill, in Yorkshire, where the clay has been thrust into the *corbicula* beds below, and adds that similar intercalated and subjacent beds of the Scottish till forming tongues are found squeezed down and between the yielding deposits; and he goes on to say: "In my brother's paper on 'The Phenomena of the Glacial Drift of Scotland' (*Trans. Geol. Soc.*, Glasgow, vol. i., part 2) will be found a section of contorted beds of sand and clay (Medwin water) which shows a similar intercalated bed of till. This bed my brother thought might have been floated as a boulder and dropped into its present position. I believe, however, that it has been injected under pressure" (*Great Ice Age*, 3rd edit., p. 362 and note).

This shows how much caution is necessary in definitely deciding whether a particular bed is really and truly intercalated between two others in districts where there has been great disturbance. It also shows how easily the strict uniformitarian will adopt an argument from the enemy when some stubborn fact refuses to fit in with his *a priori* theory. It is plain at all events that if a case is to be made out for intercalated climates it must be by stronger evidence than that of organic boulders.

Let us first turn to an area which has been examined with

much greater care and minuteness and where the evidence has been sifted in a much more careful manner than elsewhere, namely, our own islands, and we will begin with Scotland. The earliest discovery of mammoth remains in Scotland took place in the year 1817. "When," says Sir A. Geikie, "the till covering the sandstone at the quarry of Greenhill, in the parish of Kilmaurs, in Ayrshire, had been partially removed there were found at a depth of seventeen and a half feet from the surface two elephants' tusks. . . . The matrix in which they were found was a clay, which around the bones changed from its usual light brown colour into a dark brown, with a most offensive smell when turned over. The tusks lay in a horizontal position with several bones near them. Dr. Scouler visited the quarry about 1840, and reports that seven more tusks had subsequently been found there" (*Trans. Geol. Soc.*, Glasgow, vol. i., part 2, pp. 68, 69). "Some antlers of the reindeer were also found along with an elephant's tusk at this quarry" (*ibid.*, p. 71).

These remains were found in a clay containing, *inter alia*, *Astarte compressa* and *Leda pygmæa*, with eight genera and nine species of *Foraminifera* and five genera and ten species of *Ostracoda*, and also a number of seeds of plants and fragments of beetles. The earlier writers described this clay as boulder clay. Dr. Bryce, in 1864, opened some pits at the place, and found that the beds in which the remains occurred *underlie* the till, and he put them on the same horizon as "the Forest bed". Mr. Craig and Mr. Young, in 1869, after a close examination of the district and the position of the beds in regard to the boulder clay, satisfied themselves that the mammoth and shell bed were pre-glacial (see *Trans. Geol. Soc.*, Glasgow, vol. iii., p. 310). The officers of the Geological Survey in their commentary on sheet 22 describe the bed as interglacial, but after sifting the evidence again, and writing in 1887, Mr. Craig says: "I see no cause to change the conclusions arrived at in our joint paper of 1869. Dr. Bryce's surmise that the fossiliferous bed may be the equivalent in age of the Cromer Forest bed is, in my opinion, not far from being correct." Elsewhere he says he places the beds at the very base of the glacial deposits (*Trans. Geol. Soc.*, Glasgow, vol. viii., pp. 213-223).

According to Mr. J. Geikie, these remains "occurred in a peaty layer between two thin beds of sand and gravel, *overlaid by till or boulder clay*, and resting directly on the sandstone rock of the quarry" (*Great Ice Age*, p. 605).

A horn of a reindeer was also found in the basin of the Endrick, in the parish of Kilmarnock, about four miles from Loch Lomond. "The section in which it occurred," says Sir A. Geikie, "consisted first of the vegetable mould, then of a stiff clay twelve feet thick, containing a large quantity of stones, underneath which was a bed of blue clay about seven feet thick, *at the lower part of which, close upon the* underlying sandstone, the antler was found, and near it a number of marine shells. The deposit was estimated to be about 100 to 103 feet above the sea level" (*Trans. Geol. Soc.*, Glasgow, vol. i., part 2, pp. 70, 71; see also Dr. Smith, *Edin. New Phil. Journ.*, New Series, vol. vi., p. 105).

The next locality where mammoth remains occurred in Scotland was during the excavation of the line of the Union Canal, between Edinburgh and Falkirk. This was in 1820. A large mass of boulder clay having been undermined fell into the cutting, and among the earth was found a tusk measuring thirty-nine inches in length. Mr. Bald, to whom the discovery was reported, examined the place. He says it was found from fifteen to twenty feet from the surface. He did not actually take it out of the clay, and only judges that it must have come out of it from its excellent state of preservation (*Trans. Wernerian Society*, vol. iv., p. 60). It has subsequently been described as having been imbedded in the heart of the stiff clay. If it had been so, it can only be treated as a boulder, since it was quite detached. This is suggested by Mr. Bald, and it in no way evidences an old land surface; but it seems to me there is no warrant for describing it as having come out of "the heart of the stiff clay".

A skull of *Bos primigenius* was found in 1868, near Croft Head, in Renfrewshire, embedded some few feet deep in a soft clay or mud, interlaminated with lines and beds of sand and occasional layers of fine gravel. In some of the layers of clay there was a little vegetable matter in a state of decay.

These beds were *overlain* by till full of scratched stones (Geikie, *Geol. Mag.*, vol. v., p. 393, etc.).

In a paper on this deposit, in the seventh volume of the same magazine, Mr. Geikie says that it subsequently yielded remains of the horse and Irish deer. He reaffirms the opinion that the overlying clay in this instance is the true boulder clay, that it is *in situ* and in no sense due to a landslip.

On the 6th of March, 1879, there was exhibited before the Geological Society of Glasgow a well-preserved molar of a mammoth found four years before in sinking a pit on Mainhill Farm, near Baillieston, east of Glasgow. It was found in a bed of purely laminated sandy clay, at a depth of thirty-three feet below the present land surface, the sand bed being from forty to forty-five feet thick, and resting directly upon the rock-head or coal-measures without any intervening till or other superficial strata. In cutting the line of railway leading to the pit the sand bed was seen to be overlapped by a thick bed of stiff dark-coloured boulder clay full of large, travelled stones, which thinned away as the pit was approached and the sand bed rose to the surface. Mr. Young remarked that here we had another instance of the occurrence of the mammoth in Scotland during pre-glacial times, and he went on to remark that in all the cases which had occurred in Scotland the mammoth remains "had either been derived from pre-glacial beds below the till or from the till itself. Dr. Geikie says he has never been able satisfactorily to show that the mammoth-bearing interglacial beds, in any of the places where they have been found, rested on an older boulder clay, although he says that at other spots, such as Kilmaurs, intercalated beds are found in the same district between the two tills; but unfortunately for his contention, these beds have never as yet yielded any mammoth remains, nor any of the other organisms found associated with them. When traces of the mammoth have been got in boulder clay, they may in all probability have been derived from the denudation of pre-glacial beds in the same district" (*Proc. Geol. Soc.*, Glasgow, 1878-79, p. 291).

In January, 1882, there was exhibited before the Glasgow Geological Society a fragment of a tusk of the mammoth which had been found in sinking a pit on the farm of

Drummuir, Dreghorn. This was found in a bed of sand *underlying* seventy-six feet of boulder clay (*Trans. Geol. Soc.*, Glasgow, vol. viii., part 2, p. 213, etc.).

In a paper by Messrs. Craig and Young, in the third volume of the Transactions of the Geological Society of Glasgow, they say, *inter alia,* that if we look at the recorded instances of the occurrence of the mammoth and reindeer in Scotland, we shall find that in the majority of cases they either have been found in beds below the boulder clay or in that deposit itself. Those found in the boulder clay may have been derived from the denudation of pre-glacial beds. In the one or two instances where the mammoth has occurred in beds more recent than the till they may still have been worked out of it. Bearing on the pre-glacial age of the reindeer in Scotland, they refer to a portion of the right antler of a reindeer found in the boulder clay at Raes Gill, Carluke, Lanarkshire. "The specimen bears evident marks of transportation, its burr, brow-tyne and other extremities being worn and rounded, and the whole surface has a smooth, polished, ice-scratched appearance, exactly like that we meet with amongst the ice-worn stones of the till. It was found in till several feet thick" (*op. cit.*, pp. 310-320).

Turning from the remains of terrestrial animals to those of terrestrial plants the evidence is equally striking. Mr. Clement Reid, who is especially apostrophised in many places by Mr. Geikie in the last edition of his *Great Ice Age*, has recently published a most important paper on the subject of the Scotch so-called interglacial beds, and the paper is specially remarkable since Mr. Reid was specially selected for the duty of discriminating the plant remains upon which Mr. Geikie's arguments have been founded, and has been especially quoted by him for his facts. He is also a strong, nay, an extravagant, believer in interglacial beds.

Mr. Reid has seen reason to modify his formerly expressed views that the beds in which these plant remains occur are of interglacial age. He now says: "Subsequent work on the abundant material sent by Mr. James Bennie and other observers, and an examination of the Hailes and Redhall quarries, has convinced me that the majority of the plants found in the three most important deposits are of comparatively

late date, probably not older than the neolithic period." He then discusses them *seriatim*. First, the railway cutting at Louden Glen (*Great Ice Age*, pp. 102-104). Mr. Reid says he has not examined the ground itself, and can only speak of the plant remains; but several competent observers have already objected to the acceptation of the peat as interglacial, on the ground that the drift there is constantly slipping down the steep slope, so that the occurrence of boulder clay over the peat is insufficient to prove that the peat is the older of the two. He goes on to say that when he wrote his previous paper he remarked that the plants being all species still living in the district throw no light on the question. Since then, however, Mr. Bennie has sent him fresh materials, among which he has found two seeds of the garden or opium poppy (*Papaver somniferum*). "I have not," he adds, "the slightest doubt as to the accuracy of this determination, but it is extremely improbable that such a plant was a native of Britain in interglacial times. The whole assemblage of the plants, and the state of preservation of the animal remains, such as caddis cases, winter eggs of *Spongilla* and *Cristatella*, and fragments of *Daphnia*, suggest to me an extremely recent date for this deposit."

Secondly, in regard to Hailes quarry (*ibid.*, p. 99), near Edinburgh, Mr. Reid says the upper part of the peat contains a different set of plants from the lower, the lower belonging to a colder fauna. In regard to the upper part he says the occurrence of charcoal, of seeds of flax, fool's parsley, corn marigold (*Chrysanthemum segetum*), chamomile (*Matricaria inodora*) and hemp nettle (*Galeopsis Tetrahit*) is against the great antiquity of the deposit.

Thirdly, in regard to Redhall quarry, also near Edinburgh (*ibid.*, pp. 100, 101), he says the section of the ground was not clear when he visited it, but the slope of the ground seemed quite sufficient to cause slipping in so unstable a deposit as till. "The botanical evidence, however, was very strong, for the peat yields about fifty plants, including *Fumaria officinalis*, *Spergula arvensis*, *Linum perenne* (or L. *usitatissimum*), *Alchemilla arvensis*, *Carum carui*, *Chrysanthemum segetum*, *Matricaria inodora*, *Centaurea Cyanus*, *Sonchus arvensis*, *Galeopsis Tetrahit* and *Euphorbia Helioscopia*. These eleven species and perhaps

some of the others are probably weeds of cultivation. The flax seeds and capsules, judging from the abundance of these remains, may be the refuse of bundles of the cultivated plant put to steep in the marsh" (C. Reid, *Geol. Mag.*, 1895, pp. 217-219). These arguments and admissions are all the more forcible and remarkable from the fact, as I have said, that Mr. Reid is a believer in interglacial periods.

Let us now turn to England. No remains of pleistocene mammals have occurred, so far as I know, in the counties of Northumberland, Durham, Cumberland or Westmoreland. Of the few cases of the discovery of mammoth remains in Lancashire and Cheshire the only one, according to my friend Prof. Dawkins, in which the relation of the deposit containing them to the boulder clay is clear was the famous case of the discovery made by Mr. Bloxsom in March, 1878, in digging a shaft for the new Victoria Salt Company, near Northwich, of a fragment of a molar. It occurred at a depth of sixty-five feet in a bed of sand overlying the red Keuper marls, and overlaid by thirty-seven feet of brown boulder clay (*Quart. Journ. Geol. Soc.*, February, 1879, pp. 140, 141).

If we turn to Yorkshire the evidence is more abundant and more conclusive. We will begin with an early reference.

The Rev. W. Vernon, in a paper printed in the *Philosophical Magazine* in January, 1830, describes the finding of a remarkable collection of the remains of pleistocene animals at North Cliff, in Yorkshire. He mentions, *inter alia*, that while no entire skeleton was found, in some instances bones were found together which articulated with one another—thus the humerus, radius and ulna of the wolf were not far separated, and the coronary phalangial bones of the horse were found in regular order. The second a little below the first, and the third a little below the second. He remarks of the size of these phalangial bones as being twice as big as those of an ancient British horse taken out of a barrow in the East Riding of Yorkshire. Speaking of this deposit he says: "Above the bed of black marl, in which most of the bones occurred, large stones were embedded in the deposit transported from distant rocks, from the western hills of Yorkshire and the mountains of Cumberland. These stones correspond so en-

tirely with those of all our great beds of diluvial gravel, that if a heap of the former were laid by a heap of the latter the two heaps could not be distinguished from each other."

In a memoir on the moors, mountains and sea-coast of Yorkshire, published by J. Phillips in 1853, he urged that the lowest Hessle gravels, which rest upon chalk, and are covered by boulder clay, as well as the contents of Kirkdale cave, are pre-glacial. Writing in 1868, he says of this view about the Hessle gravels: "I am still disposed to favour this opinion. In the first place, there is no proof that these beds are marine, but a strong presumption to the contrary, from the considerable abundance of land mammalia found in them, especially *Elephas primigenius* and horse; and, secondly, beds of this order composed of chalk and flint fragments, not only are not known to occur in the midst of the boulder clay, but can hardly be imagined to exist there; and, thirdly, the boulder clay rests on them without conformity" (*Quart. Journ. Geol. Soc.*, vol. xxiv., p. 255).

Phillips continued during half a century to be the advocate of the mammoth beds being older than the drifts, and in the last edition (1875) of his geology of Yorkshire, in describing the ossiferous marls of Bulbecks, near Market Weighton, he shows that they lie directly on the red marls. In regard to their relative age he says: "It appears to be proved, both by comparison with the analogous deposits at Hessle and Bridlington, and by the superposition of the ordinary diluvium in the south-eastern part of the Vale of York, that the latest of these inundations (*i.e.*, that which laid down the mammoth beds) was anterior to the movements of waters which brought many Cambrian rocks through the pass of Stainmoor and dispersed them over the hills and valleys and antediluvian lake deposits of Yorkshire" (*op. cit.*, pp. 12-19). Speaking of the Hessle beds containing mammalian remains, he says: "As they are now covered up by a great thickness of clay and pebbles derived from a far greater distance (*i.e.*, by the drift), we count them the spoils of pre-glacial land" (*ibid.*, pp. 57, 58). Teeth of mammoths sometimes occur in the boulder clay in Yorkshire as they do elsewhere, but are clearly derivative and boulders. Phillips has remarked how the teeth occur in certain places and no other parts of the skeleton (*ibid.*, p. 74).

When bones occur under these circumstances they are always scattered and generally rolled (*ibid.*, p. 170).

In regard to the deposits in Kirkdale cave, he points out their analogy with those occurring at Bulbecks, "where glacial drift overlies the bones," adding "that the Kirkdale cavern was occupied in the pre-glacial condition of the land which is now Yorkshire was my earliest opinion, and seems still to be the most probable inference in the present state of knowledge" (*ibid.*, pp. 169-171).

This opinion about Kirkdale cavern is interesting, for the evidence is fast accumulating to show that the cavern deposits at all events are older than the distribution of the till. I should like to refer to another northern cavern, where, although no mammalian remains occurred, there seems to have been a very decided invasion of drift.

Speaking of the cavern at Stainton-in-Furness, Mr. Cameron says: "The floor of a gallery resembles the bed of a dry mountain torrent, being strictly strewn with water-worn pebbles and boulders. Soft yellow clay occurs, frequently also gravel; while again in other places there is a pavement of hard dry clay split up by cracks into octagonal-shaped masses. . . . In this gallery are also silurian boulders, often cemented together in huge masses. A few of these boulders are of a larger size than to have allowed of their entrance through the as yet only known inlet to the place. Ireleth, about four and a half miles off, is the nearest place where this rock is *in situ*, and boulders and fragments of rock are often met with, thrown against each other in the direst confusion as if impelled along by a very strong current and suddenly stopped" (*Geol. Mag.*, vol. viii., pp. 312, 313).

We will now turn to the famous Victoria cavern, where a considerable polemic arose in regard to the interpretation of the facts. It must be remembered that this discussion was before the more recent discoveries of Dr. Hicks, etc., in North Wales. Prof. Dawkins, who took the view in the paper that the remains in this cave were post-glacial, says in the discussion that he could not say whether the Victoria cave was pre-glacial or glacial, nor even define its relation to the glacial period. The age of the clays was, he said, a matter of opinion (*Quart. Journ. Geol. Soc.*, vol. xxxiii., p. 612).

Other explorers of the cavern were much more emphatic in their view. Mr. Tiddeman, who drew up the report to the British Association, distinctly refers the deposit to a pre-glacial age, and speaks of the Craven savage as having lived before the great ice-sheet (*Report of the Brit. Assoc.*, 1875, p. 173).

In a paper on the cave by the same author he tells us how a bed of tenacious clay with scratched silurian and other boulders was found underneath all the talus at the mouth of the cave, resting on the edges of the beds containing the older mammals, and dipping outwards at an angle of 40°. Mr. Tiddeman explains this as the remnant of a moraine (lateral or *profonde*) which dammed up the mouth of the cave and prevented anything but water charged with fine sediment from entering it during the glacial period. "Perhaps," he adds, "one of the strongest pieces of evidence that the older cave animals lived in this district only at a time previous to the great ice-sheet is that, so far as we know, the remains of none of them (except of *Cervus elephas*) have ever been found in any of the post-glacial deposits of this district. Though so common in the river gravels in the midland and southern counties, they are never found except in caves until we get much further south or east. Leeds is, I believe, the nearest locality where they occur. This would seem to imply that their remains were wiped off the area by the great ice-sheet, . . . and only left in the shelter of caves to which it could have no direct access" (*Geol. Mag.*, vol. x., p. 15).

Writing in *Nature* the same geologist says "a human bone or fibula was certainly found beneath glacial clay in the Victoria cave" (*Nature*, vol. xiv., p. 505). A dispute arose afterwards as to whether the fibula was human or not, but this does not affect the issue we raise. Mr. Tiddeman again says: "In the Victoria cave the surroundings are such that nothing but an ice-sheet could have sealed up with glacial clay the remains discovered by the committee. . . . The origin of the boulders, their position, the ice-scratches on the rocks hard by, all point to the time of greatest glaciation, when the whole district was covered in with ice and snow of great thickness. And the agent which closed the cavern and concealed the animals within it must have been the same

which swept the country clean of their remains all around further than the eye can reach" (*ibid.*, p. 506).

In the discussion on Prof. Dawkins' paper, Sir A. Ramsay said he thought the evidence for the existence of man in the Victoria cave before the glacial period was stronger than that against it. Prof. Prestwich thought the deposits in the Victoria cave were pre-glacial (*Quart. Journ. Geol. Soc.*, vol. xxxiii., p. 612).

The next case to which I shall refer is one in which we have not to deal with mammalian remains, but with the southern fresh-water shell, the *Cyrena fluminalis*, which marks the pleistocene horizon in other places.

In 1861 Prof. Prestwich read a paper before the Geological Society of London on the occurrence of *Cyrena fluminalis* over beds of boulder clay near Hull. He says there was previously no evidence of direct superposition to show the age of the shell. I quite agree with him that for this reason the instance in question is important if maintainable. Is it so? In the first place, in the pit where the *Cyrena* occurred in thousands, and of which Mr. Prestwich gives a section (*Quart. Journ. Geol. Soc.*, vol. xvii., p. 440), *no boulder clay at all occurred.* Secondly, in another section at Paull Cliff, near Hull, where the *Cyrena* also occurred in fewer numbers, there *was an underlying clay*, but Prestwich admits that inasmuch as it contained neither boulders nor fossils he could not feel certain about its being boulder clay (*ibid.*, p. 452). Prestwich then had some experimental borings made, but they did not succeed in piercing the gravel, and therefore, in his own words, "failed to obtain the exact proof" (*ibid.*, p. 453). Hence the evidence as tested by this locality utterly fails. Now it is curious that while Mr. Prestwich failed to find boulder clay in a definite position in regard to the gravels, Messrs. Wood and Rome did, and they say, "The gravel of Kelsea Hill, the subject of the notice of Mr. Prestwich, is (now that the ballast pit has been more extensively worked) shown most distinctly *to be overlain by the boulder clay, no less than fifteen feet of it being so exposed in one part*"; and they add that there are no means of ascertaining at present on what it rests (*Quart. Journ. Geol. Soc.*, vol. xxiv., p. 153).

Let us again turn to the evidence of the mammals. This

also seems to be very conclusive. I will first refer to the well-known memoir by Mr. C. Reid on the geology of Holderness. In this memoir chapters v. and vi. are headed "Interglacial Beds". I do not know why, for I confess I can find no evidence of their *interglacial* character.

Mr. Reid objects, on the evidence of the fossils, to Prof. Phillips treating the Hessle gravels as pre-glacial, an argument in which I do not quite follow him. He admits these gravels at Bridlington they rest directly on the chalk (*op. cit.*, p. 48), that is, have no boulder clay or true drift below them. Some mammalian remains have occurred there in a buried cliff, which has since been very carefully examined by Mr. Lamplugh, to whom I shall refer presently. His conclusion that this bed underlies the basement bed of the glacial series is confirmed by an observation of Mr. Reid, who says that a similar bed of chalk gravel, as shown in borings at Bridlington harbour, rests on the chalk, and is clearly beneath the basement clay (*ibid.*, p. 49).

Turning to the Hessle mammaliferous gravels on the Humber, Mr. Reid admits that they also unmistakably rest directly on the chalk, and are covered and overlapped by boulder clay. Yet he goes on to argue that "we have nothing to fix the age of the Hessle gravel by, and that there is no positive evidence whether another underlying boulder clay has been denuded or not". If so, why call the bed interglacial? And he goes on to say, "Prof. Phillips's reference of the Hessle gravels to a pre-glacial period *may turn out*, with fuller evidence, to be well founded". I should have said that it was conclusively proved, there being against it no stratigraphical facts, but merely an *a priori* prejudice based on some theory about the fossils.

A similar bed, but without fossils, at South Ferriby Cliff, on the south of the Humber, also rests on the chalk, and is overlain by boulder clay.

In regard to the marine gravels in which Prof. Prestwich found the *Cyrena fluminalis*, and which have yielded many mammalian remains, Mr. Reid admits that at one spot, at Kelsey Hill, a face of about twelve feet of weathered boulder clay, with small stones, can be seen *overlying* the gravel (*ibid.*, p. 54).

Mr. G. W. Lamplugh, in his paper on the drifts of Flamborough Head published some years ago, is most clear on the subject. He describes the buried cliff at Sewerby as having been formed by marine action prior to the deposition of any of the glacial beds, and as having afterwards been buried and obliterated by the accumulation of materials banked against it (*Quart. Journ. Geol. Soc.*, vol. xlvii., p. 394). In a later paper, where he enters into greater detail, he puts the Sewerby gravels, which have yielded so many pleistocene mammalian remains, distinctly *under* the whole glacial series, and notably under the so-called basement bed. He comes to precisely the same conclusion in regard to the marine shell bed at Speeton (*op. cit.*, pp. 410-412). In the discussion on this paper Mr. E. T. Newton describes the remains of mammals from Sewerby as "just such an assemblage as might be expected in an undoubted pleistocene deposit".

In a third paper, read in 1887, on the same subject, before the Yorkshire Geological and Polytechnic Society, Lamplugh demurs to Mr. Reid placing the beds under the heading of interglacial, as he thinks the evidence tells strongly towards their pre-glacial age, that is to say if the term *pre-glacial* may be applied to beds older than the oldest truly glacial deposit known in Yorkshire.

It seems to me, therefore, that the Yorkshire evidence, wherever we can test it, agrees with that of Scotland and of Cheshire in compelling the conclusion that the horizon containing remains of the mammoth and its companions is distinctly below the drift beds.

Having sifted the evidence in so far as it is available in Scotland and the north of England, we will now advance further south; and first in regard to the eastern counties. Here, as elsewhere, it is unfortunate that we so very seldom can find the beds upon which the solution of the problem depends in actual contact so as to apply the only real test, namely, that of superposition. When we can do so, it seems to me the case is conclusive.

In Mr. Jukes-Browne's *Survey Memoir* on the eastern part of the county of Lincolnshire there is only one section given in which the pleistocene mammals occur, namely, at Burgh. Here remains of *E. antiquus*, *R. leptorhinus* and *Bos primigenius*,

or of bison, were found in gravel *underlying* three to six feet of boulder clay and underlaid by a black turfy bed and by marl (*op. cit.*, p. 86).

At Little Bytham, in the same county, Mr. Skertchly found *Corbicula fluminalis*, which marks the same horizon, under boulder clay.

We will now turn to East Anglia. Recent discoveries have tended to increase rather than to solve the difficulty of fixing the exact age of the forest bed. The discovery of remains of the musk sheep, of the hyæna, the glutton, all animals characteristic of pleistocene times, seems to suggest either that the forest bed has been hitherto antedated, or that, like the Norwich Crag, it may consist of rearranged or *remanié* materials. Whatever view may be adopted, there can be little doubt now that the mammoth occurs in the forest bed. Dr. Leith Adams, writing in 1881, describes several molars found in it by Mr. Savin, which he unhesitatingly identifies as mammoth, and he adds, "I can have no hesitation in admitting the mammoth among the pre-glacial mammals of the British Isles" (*Pal. Mem.*, p. 174).

This last clause is no doubt a deduction from the fact that the forest bed, as exposed at Cromer, underlies the whole of the drift deposits.

Mr. Austen, writing in the seventh volume of the *Quarterly Journal of the Geological Society*, speaks of the beds of terrestrial origin (*i.e.*, of the land surface) exposed at Runton and Mundesley, and says the mammalian remains found *in the overlying till* have in every instance been derived from portions of the expanse of that former terrestrial surface (*op. cit.*, p. 133).

Again, Prof. Dawkins tells me that one of Mr. Gunn's elephant teeth from the Norfolk coast was striated and rubbed as if by glacial action.

It is in Suffolk, however, not in Norfolk, that the most interesting and critical test case is supposed to have been forthcoming, namely, the classical section at Hoxne first described by Prof. Prestwich in 1859, and which, for a long time, was accepted as conclusive, and is still the basis of the orthodox geological opinion about the age of paleolithic man. It seems to me that the conclusions based upon this particular instance will have to be surrendered. In the first place, the sections

given by Prof. Prestwich are most uncertain in their reading. Thus in the first and most important section *no bed of boulder clay is given at all.* In the section of the pit in which flint implements and mammalian remains were actually found, *all the beds* contain fresh-water shells or peat, or mammalian bones, and there is no trace of drift at all (see *Phil. Trans.*, vol. cl., p. 305).

Feet		
1 to 2	*a.*	Surface soil, traces of sand and gravel.
10 to 12	*b.*	Brown and greyish clay, not calcareous, used for brick-earth, with an irregular central carbonaceous or peaty seam. Two flint implements found in this seam the previous winter.
½ to 1	*c.*	Yellow rectangular flint gravel with some chalk pebbles and pebbles of silicious sandstone, quartz and other older rocks. Bones of mammalia.
3 to 4	*d.*	Bluish and grey calcareous clay in places very peaty, with wood and vegetable remains, land and fresh-water shells. Bones of mammalia.
1 to 1½	*e.*	Gravel like *c*, but smaller and more worn, with more chalk pebbles.
	f.	Calcareous grey clay more or less peaty, with fresh-water shells. Mr. Prestwich says he made a boring in this clay 17 feet deep without reaching bottom.

It is true that Mr. Prestwich and Sir J. Evans had some trenches dug in other parts of the field which showed the white and ochreous sands and gravels to be underlaid by a grey clay, but I altogether demur to treating this clay as boulder clay. It seems to me to answer to the clay marked *f* in the big section in the pit. At a distance of 150 yards from this pit is another pit where boulder clay is dug, and here we are expressly told that no other beds were exposed (*ibid.*, p. 307). Mr. Prestwich himself calls attention to the unsatisfactory character of the evidence (*ibid.*). The boulder clay caps all the hills around. Its very uneven base rests on white and yellow sands and gravel.

In 1876 Mr. Belt wrote an elaborate paper in the *Quarterly Journal of Science*, giving many sections of the ground at Hoxne, in which he sharply contested Prof. Prestwich's conclusions and avowed his opinion that the facts prove the implement-bearing gravel there to underlie and not overlie the boulder clay.

Mr. Horace Woodward, in a paper read before the Norwich Geological Society, says Mr. Belt recorded traces of chalky boulder clay in the Hoxne beds, and adds: "My colleague, Mr. Reid, and myself subsequently observed two tiny traces of

this boulder clay *on* the brick-earth in the pit in the park" (*Proc. Norwich Geol. Soc.*, part 2, p. 62).

It having been suggested that these pockets of boulder clay were brought into their position by man, Mr. Reid writes: "They seemed to have been there for a long time, for they were grassed over, and, if I remember rightly, an oak tree was growing on one of them" (*Mems. Geol. Sur.*, 50, New Series, p. 30).

The problem was in this position when in 1895 the British Association appointed a special committee to inquire into the whole question, and, *mirabile dictu*, in selecting the committee they did not place a single person on it known as an authority who was not already committed to a particular opinion. This is a strange method of dealing with controverted issues.

The report of this committee was presented to the British Association at its Liverpool meeting in 1896, and it is very well worth reading.

In the first place, not a single flint implement was found at all, nor yet a pleistocene bone. Secondly, the only real test of any value applied was a shaft dug down in a specially selected place, which selection was made quite fairly. The critical lignite bed, with the associated black loam, was speedily found. The important thing, of course, was to find the boulder clay below it. I will now quote the reporter's own words: "At a depth of twenty feet below the present surface they had sunk through paleolithic brick-earth and gravel; through beautifully laminated loams with leaves of arctic plants; through a seam of lignite and a foot into hard green lacustrine clay. The lignite and the sandy lower part of the arctic leaf bed looked treacherous and needed careful timbering; *it was, therefore, decided to give up sinking and to bore to the base of the lacustrine clay. This was reached at a depth of forty-one and a half feet from the present surface. . . . Directly the augur penetrated the clay and sank into the sand below, water rose up so as to stop work.*

Then comes the delightful comment: "There was no need, however, to bore further, as the doubtful points were already settled". Can anything be more ludicrous? The sand which was reached is called *glacial sand* in the annexed plan. This is just as arbitrary as Mr. C. Reid's habitually naming the

barren sands of the crag in East Anglia glacial sands when they are needed to support his interglacial theories. Nay, it is a good deal more arbitrary, for on page 410 we are actually told that the sandy base of bed E, that is, of the lacustrine clay above mentioned, which rests on this very sand, "was full of small derivative valves of Balani, such as are so abundant in the Norwich crag"; and the statement is further made that "possibly the crag may have been reached".

Thus we have a bed of Norwich crag labelled glacial sand, and on the strength of this sand the lignite is declared to be interglacial, and the doubtful points are declared to be settled. Is this science, or is it only glacial science? The fact that the boulder clay was not found in their pit was no doubt a very awkward fact for those who went down to Hoxne at the expense of the British Association, and in order to get round it we are told quite arbitrarily that the clay was cut through before the lacustrine deposits were laid down. It is curious if the clay was so cut that the underlying soft crag sands with the shells on them were not disturbed. What is perfectly plain is that the case of those who have been urging that the implement bed at Hoxne was proved by this excavation to be above the boulder clay utterly fails, and, so far as this hole is concerned, is completely disproved.

The rest of the report deals entirely with the bore holes, which seem to me quite ridiculous tests when applied to boulder clays, for what we want to know is not whether a clay bed exists under the lignite, but whether this is a true boulder clay *with foreign stones* in it. How is this to be ascertained with any precision by a boring rod? Let us, however, turn to the evidence such as it is.

The first thing to note is that in no single case, so far as I can judge, was boulder clay found underlying the implement beds. The succession of the beds found is as follows:—

(*a*) Brick-earth with fresh-water shells, wood and paleolithic implements.

(*b*) Gravel and carbonaceous loam.

(*c*) Black loam with leaves of arctic plants.

(*d*) Lignite with temperate plants.

(*e*) Lacustrine clay with temperate plants.

Twenty trial borings altogether were made. In 1, 2, 5, 7, 8, 11, 17 and 20 no boulder clay at all was found. In 3, 4 and 18 the clay was covered apparently by rain wash only. In 9 the chalky clay was on the surface and overlaid by nothing; after piercing it for twenty-four feet some sand was found. 6 was entirely off the line of other borings in a bank five feet below the road, and apparently altered ground. 20 was made in another bank; 10, 11, 12 and 19 were dug in made-ground, and were therefore valueless, as may be seen (apart from this fact) from the details given.

We have thus left for consideration 13, 14, 15 and 16, and in all these cases we have the remarkable fact that all the beds B, C, D and E have vanished, not one of them, or a trace of them, was found, showing that the whole ground had been denuded, if they were ever there, and therefore that the covering of the clay could not be A, which in such a case must have been the first to go, and making it absolutely arbitrary to identify these surface coverings with A at all.

With these most slippery facts our entirely biassed committee has the assurance to say that "the evidence is now perfectly clear that the well-known paleolithic implements of Hoxne are much later than the boulder clay of that district" (*Report to the Brit. Assoc.*, p. 12). The fact is, this egregious committee simply blew their own case into the air.

It is clear, therefore, that when analysed the case so often quoted, and upon which such a stupendous induction has been based, utterly breaks down, and that the evidence there, if it points a moral at all, points to the actual reverse of the one maintained by so many geologists.

Let us, however, proceed further, and, in the first place, turn to another district in Suffolk, namely, the neighbourhood of Brandon.

Mr. Skertchly affirms positively, in one of the volumes of the *Memoirs of the Geological Survey*, that early paleolithic remains are found there in a series of loams, sands and gravels *overlaid by chalky boulder clay.* "To this series," he says, "I have given the name of the Brandon beds. They are very fragmentary, but seem to occur pretty nearly all over East Anglia wherever the chalky boulder clay extends, always cropping out at or close to its base, and never in

a single instance occurring away from it. This remarkable association is only explicable on the supposition that the Brandon beds are older than the chalky boulder clay, and indeed that clay can be actually seen lying thick upon them, and often contorting them, sometimes for a mile at a stretch. Up to the present time they have yielded implements or flakes at Botany Bay (near Brandon), Mildenhall brickyard, High Lodge, Mildenhall, Bury St. Edmunds, West Stow and Culford. The first discovery was at Botany Bay, and at the time no boulder clay was visible at the precise spot, but it has since been met with, and I had the pleasure of the experience of Mr. Amund Helland when this fact was made clear. At Mildenhall brickyard and High Lodge good thick chalky boulder clay overlies the Brandon beds, whence many implements have been obtained, and at Culford, whence I dug out a good flake, in company with Mr. F. J. Bennett, the Brandon beds are worked under fifteen feet of chalky boulder clay, and can be traced beneath that deposit for the distance of a mile to the eastward" (*Age of Paleolithic Man*, pp. 67-69).

I ought to say that on a copy of this memoir in the Jermyn Street Museum there is, in the handwriting of Prof. Ramsay, the following note attached to a section: "Pit beneath boulder clay, opened for some years, and dug into again in 1878, where the flint implements were found, sent to the museum by Mr. J. C. Maynard, of Brandon" (*ibid.*, p. 68).

In regard to the beds at Mildenhall, where Mr. Skertchly found his paleolithic implements, Mr. H. B. Woodward, who has examined the locality carefully, says: "There was brick-earth overlaid by boulder clay. . . . There was no escape from the conclusion that the brick-earth was older than the boulder clay. This bed of brick-earth, and other beds of brick-earth near Thetford, interested me much, for I had come across similar beds near Norwich, which were older than the boulder clay. Sir A. C. Ramsay, Mr. Bristow, Mr. Whitaker and others have since seen the sections described by Mr. Skertchly, and have agreed with him that the brick-earth, said to contain paleolithic implements, is older than the boulder clay" (*Proc. Geol. Assoc.*, vol. ix., No. 1).

Speaking of the gravels at Ipswich in which mammalian

remains have occurred, Mr. Whitaker refers to the difficulty of classifying the gravel and in dividing it from the like deposit of glacial age *against which it ends off*. He also speaks of the boulder clay having been *found at the surface* in the town itself (*Geology of Ipswich*, etc., p. 93).

Elsewhere he writes of finding pieces of bone or of elephant's tusk, with shells, in a low cliff of buff sand and loam, lying directly on London clay (*ibid.*, p. 95).

The horizon of the famous mammalian deposit at Barrington, in Cambridgeshire, has been much discussed, and it has been very dogmatically stated on more than one occasion that the beds are post-glacial, meaning posterior to the deposition of the drifts. I cannot see any evidence of this. The sections clearly show that the bone beds are overlaid by trail, as Mr. Irving himself admits (*Quart. Journ. Geol. Soc.*, vol. xxxv., p. 670), and that they rest on the greensand and chalk-marl.

In the adjoining county of Bedford I know of no direct evidence of superposition, but inasmuch as a great deal has been made of such evidence as is there forthcoming by the advocates of the so-called post-glacial date of the mammoth and paleolithic man, it may be well to devote a few words to them, and especially to the section at Biddenham.

Mr. Prestwich has given an admirable section of the deposits in the valley of the Ouse at this point (*Quart. Journ. Geol. Soc.*, vol. xvii., p. 364). From this section it seems that the valley of the Ouse has been scooped out of the oolitic rocks known as Cornbrash. In the lowest portion of the trough so created runs the river Ouse, which has deposited a certain amount of alluvium along its course. On a higher level in the valley is the bed of gravel in which the flint implements were found. Lastly, the bounding heights of the valley on each side are capped with boulder clay, which stretches for miles in all directions (Lyell, *op. cit.*). The theory generally adopted is that the boulder clay was once continuous across the valley, and that the valley has been scooped out of it. This is the view of Mr. Prestwich, Sir J. Evans, Sir Charles Lyell, etc.

One thing is clear, if the boulder clay once occupied the valley in this way the scooping out must have been most complete, and the floor of the valley laid bare; for, as Sir

Charles Lyell expressly tells us, the two implements first found in the gravel were found "at the depth of thirteen feet from the surface and rested immediately on solid beds of oolitic limestone," and so he reprints it in his section. At this point, then, the evidence is plain that there is no superposition of the flint-bearing beds on the glacial clay. The former lie directly on the floor of the valley, and such evidence as is forthcoming at this point is of an essentially secondary and deductive character, and certainly does not warrant the assertion of Sir Charles Lyell that the Bedford sections teach us that the fabricators of the antique tools and the extinct mammalia coeval with them were post-glacial (*ibid.*, p. 217). But let us proceed somewhat further. In 1866 Mr. Flower described the discovery of flint implements at Thetford, on the Ouse. These he tells us were obtained from twelve to fifteen feet below the surface *and within a foot or less of the chalk on which the gravel rests; and some were found in some gravel filling potholes in the chalk* (*Quart. Journ. Geol. Soc.*, vol. xxii., p. 567). Here the evidence carries our case still further, for not only does the gravel lie immediately on the solid rock, but also in the potholes; so that the sweeping out of the boulder clay must have indeed been complete and profound. Let us turn to another site in the same valley, namely, at Summerhouse Hill, where Mr. Wyatt found flint implements which had come from beds containing a similar assortment of mammalian bones and land and fresh-water shells to those found nearer Bedford. He gives an admirable section across the valley at this point showing a state of things precisely the same as in the sites already referred to (*Quart. Journ. Geol. Soc.*, vol. xx., p. 185).

The boulder clay caps the ridges of Navesden and Hammer Hill, bounding the valley on either side; but within the valley itself, including the elevation of Summerhouse Hill, there is no glacial deposit. That hill divides the valley into two troughs, in each of which the drift gravels with mammalian bones and shells are found, and in each these gravels repose on the Oxford clay and that on the limestone of the middle oolite.

I am further bound to say that in Mr. Wyatt's section, published in the *Geologist* for 1861 (p. 243), and again by Mr.

Prestwich in the *Quarterly Journal of the Geological Society* (vol. xvii., p. 364), the mammal and implement bed at Biddenham appears to be overlain by drift.

Turning from Bedfordshire the only discoveries of interest in central England in the present discussion were made in Hertfordshire.

Prof. Prestwich, writing in 1858, says: "A ballast pit has recently been opened at the Watford end of the Bricket Wood cutting, and immediately south of the line, which exposes a section of much interest. The boulder clay has there almost thinned out, leaving but a seam one or two feet thick, whilst above and below it is a thick bed of gravel. . . . The lower gravel I believe to pass under the boulder clay. . . . In the ballast pit I was fortunate enough to discover in the lower gravel a few pieces of the tooth and tusk of the elephant. . . . The lower gravel reposes upon an irregular surface of chalk" (*Geologist*, 1858, p. 242).

Let us now turn to the Welsh caves. These caves are very important, since the evidence they preserve is much less liable to sophistication, and it is the fact of the existence of such evidence in a distinct form in so-called glaciated districts in England, whereas it does not so occur on the Continent and America, that makes the English evidence so valuable.

Dr. Buckland says that in 1836 he found, on the property of Mr. Lloyd, near Cefn cave, fragments of marine shells associated with the usual detritus, and inferred from the fact of Mr. Trimmer and Dr. Scouler having discovered recent marine shells and drifted pebbles over the bones in the cave, and from the admixture of the bones of mammifers with diluvium in Kirkdale, Torquay, and other caverns, either that these caves were submerged subsequently to their having been inhabited and again raised above the level of the sea, or that vast irruptions of water, apparently loaded with icebergs, had overwhelmed the country (*Proc. Geol. Soc.*, vol. iii., p. 584).

Making allowance for the geological language which then prevailed, this means that so far back as 1836 Dr. Buckland had affirmed that the mammalian bed in the Cefn cave was overlaid by drift.

Mr. D. Mackintosh, in speaking of the Cefn and Pont Newydd caves, refers to the clay in the cavern as containing angular and subangular fragments of limestone, a few polished fragments and pebbles of limestone, and a few pebbles of Denbighshire sandstone and grit, felstone, etc., and he says: "It is horizontally continuous with the upper boulder clay of the district. The clay can be traced along the plain of Lancashire and Cheshire, the coast of Flintshire, and up the vale of Clwyd. It spreads over the gently rising ground between St. Asaph and the Cefn and Pont Newydd caves, and it may be seen all round the caves, in some places filling up hollows, in others covering plateaux, and in not a few instances clinging to the face of steep slopes, or even adhering to narrow rock terraces or ledges. I have been familiar with this clay in Cheshire and Flintshire for four years, and have therefore little hesitation in asserting that traces of it, in an unmodified state, may be found at the entrances of both the Pont Newydd and Cefn caves, that in the interior of the Cefn cave, for a considerable distance from the entrance, there are indications of this clay once having filled the cave nearly, if not quite, to the roof, that in the interior of the Pont Newydd cave it maintains its unmodified character for a considerable distance from the entrance, and that in no part of these two caves has this clay been modified further than what may have resulted from the dropping of calcareous matter, from the temporary pounding back of water in the recesses or hollows, or from accumulations within the caves under conditions which may have differed from those without" (*Quart. Journ. Geol Soc.*, vol. xxxii., p. 92).

Trimmer, as long ago as 1841, showed that the deposits in the main Cefn cavern consisted of two layers separated by stalagmite, the upper one containing angular fragments of limestone, on the surface of which were sand and marl containing fragments of marine shells like those dispersed over the neighbouring district. Dr. Hicks considers that the stalagmite was formed over the bones in this cave "*before any* of the northern drift and sand with marine shells had been carried into the cave. . . . Had this cavern been formed after the northern drift had been deposited on the surface above the cavern, it is clear that some of that drift would have been

carried in through the fissures from the first and been mixed with the oldest deposit. The higher caverns on this side of the vale of Clwyd, like those on the east side, were undoubtedly formed before the glacial period, and were occupied by carnivora before the local glaciers had reached them." Ramsay said of this cave: "With Falconer I found fragments of marine shells of the drift in the cave overlying the detritus that held the bones of elephants and other mammalia". This was in 1876. The same view in regard both to the Pont Newydd and Cefn caves was held, as we have seen, by Mr. D. Mackintosh (*ibid.*, pp. 90-92).

Let us now turn to another case. The Cae-Gwyn cave has acquired a great notoriety in this controversy. The evidence it furnishes seems to me to be clear and conclusive that the paleolithic remains found in it were overlaid by drift at 135 feet from the entrance. As early as 1885 sand (like true marine sand) was found overlying the bone bed and laminated clays. This sand was examined in that year by Prof. Dawkins, who says: "I have carefully compared the sand and gravel found in the upper cave at Cae-Gwyn and the mud sometimes adhering to the bones with the glacial sand and gravel which occurs in the valley a little way above, and find that in every particular they agree. I have also compared them with the glacial sands and gravels near St. Asaph, and find that all three are composed in the main of quartz, quartzites and silurian fragments" (*Quart. Journ. Geol. Soc.*, xliv., p. 562).

Dr. Hicks was the first person to elaborately explore the cavern, in 1886 and 1888, and he tells us that in it the bone earth with its remains, including flint implements, was found covered in by stalagmite over which was a sandy gravel and clay with blocks of limestone, etc. Not a fragment of anything that could be called foreign material occurred anywhere under the stalagmite floor, while they occur in the newer and overlying deposits and everywhere in the neighbourhood of the cave. In some places the stalagmite floor was broken through and the bones were mixed with the drift. Subsequently some of the mixed drift from the western and northern areas was introduced into the cave. Eventually the cave was completely covered over by the next drift (*Quart. Journ. Geol. Soc.*, liv., pp. 85, 86).

Again, he says he found in it an earthy clay identical in appearance with the upper boulder clay in this area, especially that about St. Asaph and in the centre of the vale of Clwyd, and containing rolled pebbles of felsite, quartz, quartzite, sandstone, silurian and older rocks as in that deposit; there were also fragments of an old stalagmite floor; underneath this layer were found bones of pleistocene animals and a flint implement. *This was found "under the drift"*. "We clearly recognised," says Dr. Hicks, "the similarity between the material in the cavern and some of the deposits classed with the upper boulder clay in the area, and these, as we afterwards found, extended continuously outwards from the buried entrance to form the lower part of the drift section exposed in the field beyond.

"As in the Ffynnon Beuno cave, there were in some cases local materials at the base which had not been disturbed; but the thick stalagmite floor which had at one time covered the animal remains had been everywhere broken up and been scattered about in the drift along with the bones. . . . Marine shells are constantly found in the drift in this area, and a very distinct band containing marine shells in considerable abundance was found in the drift outside the covered entrance (to the cave), and in the lower parts of the section there were very clear signs of stratification. Mr. G. H. Morton, who visited the cave in 1897, says he stayed in the neighbourhood during the excavations for eleven days, besides other visits, and had ample opportunity of constantly observing the boulder clay as well as the sand and gravel and other beds beneath it, and he gives the section as:—

"(1) Boulder clay, six feet, overlapping the limestone about the entrance to the cave and with many species of shells near the base.

"(2) Sand and gravel, eight feet, ending against the limestone and filling the upper part of the cave.

"(3) Laminated clay, six feet, underlying the sand and gravel and penetrating the whole extent of the cave.

"(4) Bone earth, one to six inches, with fragments of stalagmite, mammalian bones, teeth and a flint flake clearly underlying the laminated clay over an area of several square yards outside the entrance and the whole floor inside the cave. . . .

The laminated clay had clearly been deposited tranquilly over the bone earth, the sand and gravel were over the laminated clay, but current-bedded as such so-called 'middle sands' often are. Finally, the boulder clay occurred over the sand and gravel without any evidences of disturbance or rearrangement of any kind. The top of the boulder clay formed the surface of a nearly level field, there being no higher ground near, from which the débris could have been derived, and there is no reason for supposing that the surface over the cave was ever deeply covered with clay. . . . The boulder clay resembled undisturbed clay as seen anywhere in the vale of Clwyd, Cheshire or Lancashire, while the erratics it contained were very similar" (*ibid.*, pp. 86-89).

Again, Dr. Hicks says: "The recent researches at Cae-Gwyn have proved most conclusively that there was no foundation for the views of those who contended that the drift which crossed over the entrance and extended into the cavern was *remanié* and had gradually crept down the hill. They have shown beyond the possibility of doubt that the deposits which overlie the true cave earth are *in situ* and are identical with the typical glacial deposits of the area. . . . The excavations carried on in 1885, 1886 and 1887 show that the caves must have been occupied by the animals before any of the glacial deposits now found there had accumulated" (*ibid.*, p. 575). Mr. De Rance writes, June, 1886: "The entrance to the cavern had been discovered, and a vertical shaft twenty feet deep disclosed boulder clay resting on drift sand which passed continuously into the cavern itself, while the underlying bone earth similarly passed outside the cavern and formed the base of the cutting as far as it was carried. In June, 1887, the pit in the drift was cut still further back, the bone earth still continuing to form the base of the glacial drift" (*ibid.*, p. 576). "Mr. Strahan believed that the drift of the mouth of the cave was part of the northern drift which he had mapped over a large part of Denbighshire, Flintshire and Cheshire, and that the bone earth lay beneath it" (*ibid.*, p. 577).

In the discussion on Prof. Hughes' paper in the *Quarterly Journal of the Geological Society* (vol. xliii.) on the drifts of the vale of Clwyd, Dr. Hicks said the Arenig drift is known from well-sinkings to be underlain by sands and gravels like those

at Talargoch, in which bones of animals similar to those found in the caverns have been discovered. He said, further, that he was perfectly convinced by the evidence found during the exploration of the caves of Ffynnon Beuno and Cae-Gwyn that they must have been occupied by man and the animals before the climax of the ice age, and that the mammalian remains and the implements must be considered as of pre-glacial age. Prof. Dawkins said that after examining the first section he felt obliged to accept Dr. Hicks' evidence. The drift above the place where the implements were found was, in his opinion, not *remanié*, but *in situ ;* with regard to the mammalia found in the caves of the vale of Clwyd nearly all were living in the eastern counties in the pre-glacial age (*op. cit.*, pp. 116-118).

Prof. Prestwich, in 1887, in abandoning his earlier view which was against the existence of pre-glacial man, said that the cave-work of Mr. Tiddeman and Dr. Hicks gives strong presumptive evidence of the earlier geological appearance of man in the British area, and he saw no reason to doubt the sub-boulderclay evidence of Mr. Skertchly. "Of the correctness of his opinion in regard to the stratigraphical position of the bed in which his specimen was found I have however little doubt" (*Quart. Journ. Geol. Soc.*, vol. xliii., pp. 406, 407). In the discussion which followed Mr. De Rance said he quite agreed with Dr. Hicks in his interpretation of the facts observed by him (*ibid.*, p. 409).

In 1896 the Rev. G. C. Pollen reported to the Geological Society on the exploration of Ty-Newydd cave, near Tremeirchion in the vale of Clwyd. In this report he says that although the ground above the cavern is strewn over with drift and erratics, granites and felsites from the north and from the central part of Wales, not a fragment of anything but immediately local material has been discovered in the cave itself, showing clearly that the deposits in the cavern had been carried in by water before the north and west ice had reached this area (*i.e.*, to construe this into my language, before the distribution of the drift); and he concludes definitely that the local deposit in the cavern is of earlier date than the boulder clay with western and northern drift. The occurrence of a tooth of a rhinoceros in the lower part of the cave

shows that the animal was contemporary with or of earlier date than the infilling of the cave by the local drift (*Proc. Geol. Soc.*, 1898, pp. 19, 20). Later he says of the relative age of the two critical beds that all doubt as to the fact of the local deposit being older than the boulder clay is now removed by our having the two beds superimposed in the second vertical shaft (*Quart. Journ. Geol. Soc.*, liv., pp. 89, 90).

Leaving Wales, Mr. Symonds, speaking of the hyæna den near Ross, situated 300 feet above the present Wye, says that he found in it a stratified red sand and silt three or four feet thick containing pebbles, one of which was of greenstone, and he adds: "Every one of those pebbles out of that red sandy deposit must have been derived from silurian and trap rocks, which are not to be found *in situ* until after we have traversed the long tract of old red sandstone through which the Wye passes between Coppel Wood Hill, near Ross, and Trewerne, above Hay in Breconshire, a distance by the river of seventy or eighty miles" (*Geol. Mag.*, viii., p. 436). The evidence of this cavern, therefore, seems to support that of North Wales.

If we now leave the area where true glacial drift is supposed to occur and enter the country south of the Thames the evidence seems to me equally conclusive.

In the Thames valley we find the trail of Mr. Fisher, which answers in time, etc., to the Northern drift (*vide* next chapter), overlaying the mammalian and paleolithic gravels of the Thames valley. Dr. Falconer, in discussing in 1857 the deposits at Gray's Thurrock and the lower beds at Brentford, inferred that they were of an earlier age than any part of the boulder clay or till (*Quart. Journ. Geol. Soc.*, xiv., p. 83). "It appears," says Mr. Symonds, "that these Thames brick-earths are covered by a glacial deposit of ice-borne débris as is the forest bed by boulder clay" (*Geol. Mag.*, v., p. 420). Mr. J. Geikie says: "In the south of England no true glacial deposits occur, but these are represented by rubble drift and 'trail,'" and he calls attention to the fact that in Sussex, pleistocene deposits with *E. antiquus* and *R. leptorhinus* are overlaid by chalk rubble, while in the Thames valley the drifts with paleolithic implements and pleistocene mammalian remains are everywhere overlaid by "trail" (*Great*

Ice Age, p. 644). Again, he refers to the paleolithic floor at Stoke-Newington, which its discoverer, Mr. W. G. Smith, describes as covered by the "contorted drift," which has in places ploughed it up and sometimes pushed under it (*ibid.*, p. 639).

Professor Boyd Dawkins, in his paper on the age of the lower brick-earths of the Thames valley in 1867, gives a section from the Uphill pit at Ilford, in which the famous head of the mammoth now in the British Museum was found. The bed in which it occurred was covered by several beds of loam, clay, gravel and sand, of one of which Prof. Dawkins says: "By the comparison of its bedding, the admixture of clay with sand and gravel, and the presence of pebbles of chalk and of large transported boulders of grey wethers and of flint, it is proved beyond doubt to be of glacial origin and to have been carried down by the ice and deposited, on its melting, upon the eroded top of the fluviatile deposits below". In regard to Mr. Prestwich's notion that the bed was partially formed from gravel derived from the boulder clay, Mr. Dawkins says: "A careful examination compels me to believe that there is no proof of the derivation of this glacial deposit from the wreck of the boulder clay" (*Quart. Journ. Geol. Soc.*, vol. xxiii., p. 93). Turning to Gray's Thurrock, he argues that "we have precisely the same superposition, there being a bed (numbered 6 on the section) which from the irregular size of its pebbles, its tabular flints, its contortions, and its irregular deposition owes its presence to ice in some form or other. Its sandy nature may be owing to the Thanet sand having been caught up by the ice and deposited on its melting, just as the clayey nature of the trail at Ilford was probably owing to portions of the London clay being in like manner transported" (*ibid.*, p. 95). This bed was distinctly superimposed on the gravel containing mammalian bones. At Crayford he gives a section in which a similar bed, number 7, consisting of an irregular reddish-sandy contorted stratum, full of large flints both angular and water-worn, and of quartz pebbles, and confusedly bedded, is found in just the same situation as the drift beds above named (*ibid.*, p. 96). At Erith, Mr. Dawkins discusses an interesting section, in which he found the same superposition; and he specially remarks

upon the presence of a large lump of black clay, with its angular shape preserved, which had been transported about 150 yards and deposited in the trail, and says of it: "It is altogether impossible that this angular mass of clay could be transported more than 150 yards, preserving its angularity and deposited in such a matrix, by any other agency than that of ice. The tract here was highly contorted, contrasting much with the horizontal beds below it" (*ibid.*, p. 98). He concludes that the beds at the several points described are contemporary, and that they establish that after the temperate conditions marked by the mammalian remains they were followed by a period of intense cold, in which stones, sand, clay, and indeed whatever came within the reach of the ice in the neighbourhood, was caught up and deposited in a most confused jumble on its melting (*ibid.*, p. 99); and in summing up the case he puts the lower brick-earths of the Thames valley distinctly below the glacial beds (*ibid.*, p. 109).

In discussing Mr. Dawkins' results, Mr. Marr, in the *Geological Magazine*, suggests that the beds of the Thames valley containing transported materials may be an inland extension of the boulder clay *on the same level* (*op. cit.*, vol. iv., p. 100).

Again, we have the most recent discovery of all, so lately discussed before the Geological Society by Dr. Hicks, where an old land surface containing considerable unweathered remains of the mammoth was found in the heart of London reposing directly on the London clay, overlaid by a clay containing drift from Hertfordshire, and filled with chalk, which Dr. Hicks, as I think, most conclusively correlates with the chalky boulder clay.

Lastly, we have the bone bed on the south coast, of which Mr. Godwin Austen says that large portions of the skeleton of *Elephas primigenius* with a number of shells were found underlaid by red gravel and overlaid by a few feet of deposit containing some large angular erratics (*Quart. Journ. Geol. Soc.*, xxvii., p. 17).

Many years ago I wrote: "I have now examined every instance known to me where it is possible to test by superposition the question of the relative age of the mammoth beds and the drift in the British Isles, and I claim to have shown that, as tested by these islands, the mammoth beds

when *in situ* are in every instance overlaid by the drift and are never underlaid by it".

To this conclusion Dr. Hicks when president of the Geological Society gave his emphatic adhesion, and the burden of a large part of the address he delivered as president was devoted to pressing it home. His conclusion, in his own words, was: "From numerous examinations made of undisturbed glacial deposits in Wales, the north of England and Scotland, it has been proved very clearly that the extinct mammalia, whose remains are found in association with the implements of paleolithic man in caverns, must have lived there before these deposits had been laid down, as their remains always occur at the base or in the lower parts of the drift and never above it. Further, there is not a particle of evidence to show that these extinct mammalia ever revisited those areas after the close of the glacial period" (*Quart. Journ. Geol. Soc.*, liv., pp. 101, 102).

This was a very welcome pronouncement to myself, who had fought for this conclusion unheeded for many years. It has recently been supported by two influential geologists. Speaking on Mr. Pollen's paper, Mr. Strahan said: "That the cave was filled in pre-glacial times was proved by the total absence of material foreign to the district. An interglacial episode would not explain the circumstances; the contents of the cave must have been formed before any of the drift with erratics, which is now so abundant, reached the neighbourhood." Mr. Marr declared that the deposit of materials yielding a definite fauna in caverns before the last glaciation of North Wales and the north of England seemed now to be satisfactorily established as the result of the work by Mr. Tiddeman on Victoria Cave, Settle, by the President on the caverns adjoining those described in this paper, and now by the author of the paper just read. The evidence was cumulative, but he believed that had the question of the age of man been left out of account the evidence long ago brought forward by Mr. Tiddeman would have been generally accepted (*Proc. Geol. Soc.*, 1898, p. 21).

Let us now leave England and cross the Channel. In Belgium and Northern France the true boulder drift does not occur, and we have to fall back upon beds like the trail, etc., in the south of England.

In the first place, I would mention that Prof. Prestwich, who had previously assigned the paleolithic gravels of the Somme, etc., to a post-glacial age, subsequently changed his view, thus: Dr. Hicks says "Prof. Prestwich, our greatest authority on these questions, has recently stated that, in consequence of the newly accumulated evidence showing the occurrence of human relics in glacial times, he has been led to change his views as to the age of the high-level gravels in the Somme, Seine, Thames and Avon valleys, and that he is now disposed to assign these beds to the earlier part of the glacial period" (*Trans. Herts Nat. Hist. Soc.*, v., p. 253).

M. Lapparent says: "*Le sable de la Campine*, équivalent du diluvium sableux de la Néerlande et de l'Allemagne du Nord, reposait *sur* le limon Lesbayen à *Elephas primigenius*" (*Géologie*, p. 1105).

In regard to the cavern of Chaleux, on the Meuse, M. E. Dupont writes that the remains of man were deposited before the deposition of the yellow clay with angular blocks of limestone and the deposition of the loess (*Quart. Journ. Geol. Soc.*, xxiii., "Translations and Notices," pp. 3, 4). This is also true of other Belgian caverns.

Let us now turn to the Alpine district and the north and north-east of Europe. We will begin with the Alpine country, which occupied us considerably in this behalf in a former work (*Glacial Nightmare*, p. 463, etc.).

If we begin with the beds south of the Alps we shall be struck on turning to Prof. Geikie's *Great Ice Age* by the considerable change which has taken place in his treatment of the position between the second and third editions of that book. There occur to the south of the Alps a series of lignite beds to which I referred in my former work (*ibid.*, p. 467). These beds, according to the Italian geologists, especially Gastaldi, rest upon pliocene deposits and are covered by diluvium, and this latter fact is admitted by Mr. James Geikie in the second edition of his work, where he says: "It is true that so far as is known no glacial deposits underlie the Italian lignites" (*Great Ice Age*, new edit., p. 431).

Gastaldi puts these lignites on precisely the same level with certain bone-bearing beds, such as those occurring in the Val Borlezza, where *Rhinoceros hemitœchus* has occurred. There

has also occurred there a series of plants, including *Magnolia, Acer Pseudo-Platanus, L. var. paucidentata, Buxus sempervirens, Ulmus campestris, Taxus baccata* and *Phacidium buxi* (*Great Ice Age*, new edit., p. 532). This bed is therefore of pliocene age, and it is underlaid by beds containing so-called glaciated stones. We can hardly doubt that these stones are derived, as Gastaldi argued, from the miocene conglomerates of the district and have nothing to do with the so-called glacial age at all. Similar beds occur at Leffe in the Val Gandino. Here, as may be seen from the section given by Mr. Geikie, there is no question of any glacial débris at all, the lignite containing plant remains being overlaid by torrential and diluvian beds only and having no so-called glacial bed below it (*ibid.*, p. 535). This being the evidence from Borlezza and Gandino it is characteristic of Prof. Geikie that he should clinch it by a tremendous assertion. "*In short,*" he says, "*we have here clear evidence of two glacial epochs separated by a long interglacial stage.*"

It is curious to compare this last edition of his well-known book with the second one in regard to the supposed interglacial beds of North Italy. In the latter work the marine sands occupy a considerable place, in the former they have disappeared altogether. Now this is very curious, for the marine sands in question in places overlie the mammalian beds above referred to, and are classed by the Italian geologists as pliocene and therefore pre-glacial, and this from the proportion of extinct forms they contain. This is especially the case with the shells found at Cucciago (on the Milan and Como railway) and at Ronco near Cassina Rizzardi (see *Great Ice Age*, 2nd edit., p. 441, cut out of the third edition). Now we are expressly told that mixed with these shells *were striated stones, some of which had been drilled by boring molluscs,* and also disc-shaped pebbles, that is, shingle pebbles. These striated stones are the kind of stones relied upon by Mr. Geikie in his argument, and they furnish some matter for comment, for if they were bored by molluscs they must have been submerged for a long time and the striæ upon them may have been caused by the moving of shingle, etc. It at all events proves that in their present condition the stones are older than the pliocene sands in which they lie. We are

expressly told that striated stones occurred in the pliocene clay which was well stocked with its characteristic shells. Mr. James Geikie adds: "No doubt is expressed by the Italian geologists that the shells really belong to the so-called pliocene. Out of fifty-three species obtained from Cassina Rizzardi the late Signor Spreafico found that thirty-one were extinct, twenty were still living in the Mediterranean and two in tropical seas; so that if we compare the shells with those that occur in the sub-Apennine strata we cannot refuse to class them as of the same age" (*ibid.*, p. 441). None of the shells are arctic, and the majority distinctly point to warmer conditions. Similar striations on stones drilled by boring molluscs are described by Neumayr from Ronco, and he calls them 'scratched glacier stones' without any scruple.

Gastaldi in his memoir in reply to Mr. James Geikie, which is admirably written and argued, seems to me to show very plainly that the deposits of bones of pleistocene beasts from the southern flanks of the Alps are of the same age as those of Dürnthen and Utznach in Switzerland, and are also of the same age as the lignites of Carignano, Lanzo, Gifflenga, Boca and Maggiora in Piedmont and of Leffe in the Borgamasco.

He goes on to say that the lignites of Leffe, of Boca, of Gifflenga, etc., are at the base of the diluvium (*i.e.*, of the so-called glacial beds) and overlie the pliocene beds. In Switzerland there are no pliocene beds, and the beds at Dürnthen, Utznach and Vangen rest immediately on the molasse. Our lignites, he adds, and those of Switzerland occupy the same horizon at the base of the diluvium (*Atti Acad. delle Scienze di Torino*, viii., p. 439).

Geikie professed not to have been convinced by Gastaldi's statements, but it is a very curious and eloquent fact that the Italian evidence, which in the older edition of the *Great Ice Age* occupied the whole of chapter xxxiv., has been almost entirely cut out of the new one, and we may perhaps interpret this fact as meaning that Mr. J. Geikie is no longer satisfied that the arguments there used are any longer good. It is not very ingenuous of him to cover his retreat by the mere remark in the preface that the account of glacial action in Alpine regions has been "touched up". Touched up means, I pre-

sume, cancelled and withdrawn. We must in fact go back to Prof. Geikie's first view of all (see *Geol. Mag.*, 1872, ix., p. 256), to which he has reverted, and which is unquestionably right, namely, that North Italy presents us with no evidences of former oscillations of climate.

Let us now turn to the valleys of Switzerland itself and its borders. Here, again, it is remarkable, but in another way, and not very pleasant to read Mr. James Geikie's description of the Zürich evidence, which he again quotes as if it were intact instead of having been riddled through and through by Heer himself, by Escher von der Linth, Lyell, Charles Grad, Mortillet and Neumayr, all of whose observations and arguments on the point are ignored by the author of the *Great Ice Age*, and this greatly sophisticates the value of the remaining evidence he produces. He cites the beds at St. Jacob in the Birsthal and those at Chambery and Sonnaz as if they were interglacial, in the teeth of the positive statement of Neumayr that in none of the places in question do the old land surfaces occur intercalated between glacial beds (see *Glacial Nightmare*, p. 465).

The fact is that wherever in Switzerland the sequence has been actually ascertained by superposition the beds containing pleistocene remains always underlie the erratic beds.

Thus a skull of the mammoth was found in 1841 four kilometres from Rappersschwyl in the canton of St. Gallen. According to the section published by Escher von der Linth and C. Martins in the *Bull. Soc. Géol. de France*, 2nd series, 1858, vol. vii., p. 601, the beds lay in the following order :—

(1) Black and angular fragments of glacial origin.

(2) Rounded pebbles and boulders, sometimes as big as a man's head, like those in the Nagelfluh.

(3) Bluish and yellowish clays.

(4) Bituminous remains of wood mixed with sand and clay.

(5) The mammoth's skull in question.

(6) Greyish clay and fine sand containing *Planorbis*, *Paludina* and *Cyclas*.

(7) Molasse and Nagelfluh of tertiary age.

According to the same authors bones of the mammoth have been found by M. Nicollet in the valleys of the Jura at Neuchâtel in a thin stratum of diluvial gravel composed

of jurassic and tertiary pebbles and covered by glacial débris consisting entirely of Alpine rocks (*ibid.*, p. 602).

In regard to the beds at Utznach and Dürnthen I discussed them at considerable length in my former work (*Glacial Nightmare*, pp. 463, 464). I have little more to say about them. Mr. J. Geikie admits that they lie directly on highly disturbed deposits of miocene age.

"The lignite beds of Utznach and Dürnthen," says C. Lewis, a most ardent glacialist, "supposed to indicate a warm inter-glacial epoch, are in reality pre-glacial and correspond with the pre-glacial forest bed of Norfolk of quaternary or pleistocene age" (*Glacial Geology of Great Britain*, p. 25). Prof. Boltzer, of Berne, told the same writer that he was unable to find the two tills at Dürnthen (*ibid.*, p. 461). I have also sifted the evidence at Wetzikon and shown how worthless it is (*Glacial Nightmare*, pp. 464, 465). I may add to what I then said that Gastaldi suggests that at Wetzikon and Morschweil the supposed underlying boulders are really tertiary boulders, and he calls attention to the existence in Switzerland as in Italy of miocene boulder beds from which these stones may have come. In regard to the other cases of supposed inter-glacial beds, based on the evidence of the intercalated mammals in Switzerland and its borders, I have nothing to add to what I said in my *Glacial Nightmare*, pp. 465, 466.

The most famous pleistocene beds of Savoy, namely, the lignites of Sonnaz, distinctly underlie glacial deposits, while Favre says that no glacial bed has been found below the lignites of Savoy or that of the Bois de Batic.

Let us now turn to the famous bed at Hötting, opposite Innsbruck, about which so much has been written and said, and about which so much remains to be written and said.

The contest has raged round the curious breccia which has been claimed by Penck as such a notable example of an inter-glacial bed, and I discussed it in my former work (*ibid.*, p. 469). There are two matters in dispute: first, the age of the breccia, and, secondly, the question of whether it lies between two so-called glacial beds.

First, as to the age of the breccia.

In 1859 Pichler found some plant remains in it which were described by Unger as *Arundo Goepperti*, a species of *Cyperites*,

Persea speciosa, Acer trilobatum, Ulmus Braunii, Laurinea and *Laurus*, and he assigned it to miocene times.

In 1884 Von Ettingshausen, having obtained some fresh materials, went over the ground again, and, while retaining the *Arundo* and the *Cyperites* of Unger, described the *Acer* as *Acer Pseudo-Platanus*, united the *Persea, Laurinea* and *Laurus* into one new species which he called *Daphne Höttingensis Ett.*, and identified other specimens with several living species, and he concluded that this flora might well be pleistocene.

Stur, who acquired some new materials, reverted to the view maintained by Unger as to the tertiary age of the bed in question, and claimed to have discovered, *inter alia*, remains of a palm. He classed the *Cyperites* of Unger as a *Chamærops*, and Ettingshausen's *Daphne* became with him an *Actinodaphne*. In 1887 Palla wrote on the leaves in question; restored *Chamærops* to the genus *Cyperites*, and again treated the remains as pleistocene. In 1888 Wettstein argued that the *Actinodaphne* of the above-named writers was really *Rhododendron ponticum*, and also classed the flora as pleistocene. Lastly, in 1894, Rothplatz, in his *Geologischer Querschnitt durch die ost Alpen*, pp. 95 and 96, subjected the previous opinions to criticism, and specially appealed to the specimens preserved in the collection at Munich. He remarks that Wettstein in his synopsis of the Hötting flora classes thirty-five species out of forty-one, or eighty-five per cent., as living, and six, or fifteen per cent., as extinct, and thus (taking his own identifications) gives us too large a proportion of extinct forms for a pleistocene flora. Among the forms, again, which he allows to be living, six, or fifteen per cent., no longer live at this elevation above the sea, while the *Rhododendron ponticum* requires a mean climate 10° C. hotter than that of the present Inn valley. This, he says, again speaks against the interglacial character of the flora, since it bespeaks a climate much warmer than any known in pleistocene times in other parts of Europe.

Rothplatz then goes on to question some of Wettstein's identifications. He says it seems to him very improbable that all the long leathery leaves which are preserved in the Munich museum belong to *Rhododendron ponticum*, and believes that

some of them belong to some unrecognised species or genus, and he says that they in fact resemble similar leaves which have been found in tertiary beds. Again, he says he had himself found among the leaves those of *Fragaria Haueri*, which Wettstein does not mention, and which are very like those of *Fragaria vesca* described by Stur from the Sarmatian or younger miocene. Similarly, the miocene or Sarmatian *Acer Juronaky* is probably to be identified with *Acer Pseudo-Platanus*. Lastly, the fossil leaves described by Wettstein as *Bellidiastrum Michelii* seem to be identical with those named *Parotia pristina* by Stur from the Sarmatian beds (Rothplatz, pp. 95, 96).

All these facts go to strengthen, if not to prove, the conclusion which I maintained in my former work, that the breccia in question is a tertiary bed and has therefore nothing to do with any interglacial horizon.

Let us now turn to the stratigraphical evidence. Here, again, I will appeal to Rothplatz, who has made the most recent and critical examination of the ground. He says of Blaas' sections and memoir in volume xlvi. of the *Jahrbuch der K. K. Geol. Reichanstalt*: "Among the careful researches which Blaas has published it may be clearly seen that there is only one place where the underlying of the Hötting breccia by a moraine can be actually seen, and even this is most doubtful". He goes on to say: "I have this spring (*i.e.*, in 1894) examined the site above the castle at Weiherburg." This is apparently the best exposure and specially referred to by Prof. Geikie. He gives the result in a section. Penck gave a similar section of this plan in 1882, and Rothplatz goes on to say that his Fig. 1 on Plate 2 shows a division in the moraine, which is certainly not there. The sandy layers given in Fig. 2 are certainly there, but they are altogether a local circumstance. The moraine certainly extends from the vale at this place under the breccia, but not as if the moraine was first deposited and upon it the breccia, but rather as if the breccia had been undermined and then the moraine matter had been thrust in. This is supported by the fact that the horizontal layers of the breccia are sharply cut off against the moraine which comes up against it. It is further supported by the fact that although the moraine matter is crowded with crystalline erratics from the central Alps they do not

appear in the breccia at all, or so seldom that after a patient search he could find none. If the moraine matter were older than the breccia it ought to show marks of erosion at the point of contact, and it also ought to contain derived stones, which it does not. On the other hand, the breccia is here full of pieces of red sandstone, which are entirely wanting in the moraine. Penck claims to have found numerous primitive boulders in the breccia; Rothplatz could find none. "If there were any, however, we must remember that the valley of the Inn takes its rise in the crystalline mountains, and that they would, if the valley existed in tertiary times, naturally find their way into its gravels," etc. Rothplatz goes on to argue that the beds above the Weiherburg cannot therefore be accepted as by any means clear evidence of an interglacial period, and when we also take into consideration the paleontological evidence which points to a tertiary horizon the improbability of these being so becomes great. Blaas wished to ascertain the fact by the crucial experiment of digging a shaft above Weiherburg, but was not permitted by the owner of the ground. For the view that the moraine is really an ancient deposit from a glacier which has been washed under the hollowed breccia may be cited the fact that the ancient glacier of the Sell abouched into the Inn valley opposite this place, and it is very probable that its wash would reach thither (*op. cit.*, pp. 96-98).

Blaas himself, as we have seen, wished to test the question by digging, and he further expressly says in regard to this section: "In the meantime we cannot certainly say whether the moraine underlies the breccia or not" (*Jahrbuch der K. K. Geol. Reichanstalt*, 1890, p. 48).

Let us now turn to North Germany. Dr. Jentsch, who is a strenuous believer in a German interglacial age, speaks of the difficulties there are in investigating the surface beds of Germany. "Most of our provinces," he says, "are poor in pleistocene fossils, and in most of the provinces these fossils are mostly found not in their original place, but in their second or third place of deposition; often the bones of great terrestrial mammals are mixed with fluviatile or marine molluscan shells. Our drift deposits have a thickness of 100 to 150 metres, and their stratigraphy is very complicated and variable

within small areas. We have not only two, but four or five or more glacial deposits (boulder clays) at the same locality separated by stratified beds of gravel, sand and clay. Beds of the same character are often found at very different stratigraphical levels, and they are often disturbed by glacial pressure and otherwise." He also speaks of certain deposits where the fossils are not consistent, for he says in these ancient glacial deposits teeth of *Elephas* occur near shells of *Yoldia*, and *Yoldia* is mixed with *Dreissena* or *Valvata*. Again, speaking of what he calls the younger gravels, he says the fossils are mixed, representing all known pre-glacial and inter-glacial beds. Very commonly we find *Yoldia arctica* together with *Cardium*, *Dreissena* and *Elephas*, showing clearly that these fossils must be regarded as derived from older beds (*American Geologist*, xiii., p. 222).

This shows how difficult it is to read the evidence rightly unless it is very closely watched. To my mind the result of the critical analysis of the finds in Germany as in England is to make it certain that the mammoth and its associated fauna, including paleolithic man, are older than the distribution of the drift, and wherever the evidence seems to point the other way it is because it has not been sufficiently examined. The same is the case, as it seems to me, with the beds of fresh-water marl containing a large number of species of fresh-water shells, and notably *Dreissena polymorpha* and *Valvata piscinalis*, which correspond to the *Cyrena fluminalis* in our own beds in that they are now extinct in North Germany. They were entirely contemporary with the mammoth there, and I believe they became extinct together at the time when the drift was distributed. About a large number of these beds there is hardly a dispute. They are confessedly what, in the current jargon of geology, are known as pre-glacial beds.

The facts about the surface beds round the Frische Haff have been well condensed by Mr. James Geikie. He says: "*Under what is considered to be the lower boulder clay of the low-lying Baltic coastlands* of East Prussia, as at Steinort, Reimannsfelde, Lenzen, Succase and Tolkemit (all on the Frische Haff), there occur certain bedded clays which have yielded arctic and boreal shells (*Yoldia arctica, Astarte borealis, Cyprina islandica*), the arctic seal (*Pagophilus grœnlandicus*), a cetacean

(*Delphinus*, species), and an undetermined form of fish, one of the *Gadidæ* (cod family). Closely associated with these marine clays are fresh-water beds with shells (*Dreissena polymorpha, Valvata piscinalis, Unio*, species), mammalian remains (*Canis familiaris*, var. *grœnlandica, Ursus*, species, *Elephas*, species, *Bos*, species, *Bison*, species, *Cervus tarandus, Cervus*, species, *Rhinoceros*, species, *Equus*, species), diatoms and quantities of wood" (*The Great Ice Age*, 3rd edit., p. 441).

In a paper on *pre-glacial* fresh-water beds in the diluvium of North Germany, Keilhack describes certain beds at Belzig, on the Berlin-Wetzlar Railway, containing shells of *Pupa muscorum, Vertigo antivertigo, Vertigo pygmæa, Helix pulchella, Achatina lubrica, Valvata macrostoma, Limnæa minuta, Planorbis marginatus, Planorbis lævis, Pisidium nitidum* and *Cyclas cornea.* The numbers of these shells, he says, is enormous. There were also found there remains of the red-deer and of perch, carp and pike, of insects, and leaves of the alder, maple, pine, willow, *Carpinus, Betula* and *Cornus sanguinea.* He gives several profiles, and makes it plain that his conclusion is right that the bed in question *is pre-glacial.*

He found similar beds at Uelzen, in Luneburg. Here were found remains of deer and *Bos*, and of a tree, probably *Pinus silvestris*, of the perch and carp, and numbers of fresh-water shells.

Again, at Gözzke, in Saxony, there are similar beds of fresh-water marl underlying drift, of which he gives a section (*op. cit.*, p. 153). In one of the sections there Hoffmann and Klöden describe remains of mammals, of fish and of fresh-water shells, notably two kinds of *Planorbis* and a *Unio* and numerous plant remains. Keilhack also found there *Valvata piscinalis* (var. *contorta*) and fragments of a *Limnæus*, probably *palustris.*

In similar beds at Korbiskrug, Dr. Laufer found remains of *Cervus elaphus* and remains of the perch and a kind of carp. Of shells, *Valvata piscinalis, Bithynia tentaculata, Planorbis lævis, Limnæus auricularis, Pisidium amnicum* and *P. pusillum.*

Again, at Bienwalde, in Bradenburg, Klöden describes a similar bed containing remains of pike, perch, carp and remains of a small mammal (? *Hypudäus arvalis*), with shells.

At Oberohe, in the Luneburg Heath, Cleve and Jentsch found a bed containing diatoms in the same position, *i.e.*,

below the so-called glacial beds. Keilhack gives a long list of these diatoms. Ehrenberg also noticed among the latter remains of fungi and of the pollen of pine trees. In addition, Prof. Berendt describes from the same beds remains of *Quercus robur*, *Q. sessiflora*, *Fagus sylvatica*, *Betula alba*, *Alnus glutinosa*, *Salix*, species, *Populus*, species, *Myrcia*, *Vaccinium Myrtillus*, *Acer campestre*, and *A. platanoides*, *Utricularia*, species, *Pinus sylvestris*, cryptogams and fish.

In regard to the horizon of these beds Keilhack says: "Sowohl Lagerungsverhältnisse wie organische Resten hinweisen dass die beschriebenen Ablagerungen in der That præ-glacial sind" (*Jahrbuch der Königl. Preuss. Geol. Landesanstalt*, etc., 1882, pp. 133-172).

From fresh-water marls in the same district (North-East Hanover) on the same horizon Dr. Laufer obtained numerous relics of plants and animals, among which are the following forms, hitherto unknown from this horizon in Western Germany. *Rhinoceros*, species, *Emys europæa* (tortoise), *Abramis brama* (bream), *Helix austriaca*, *Populus tremula*, *Corylus avellana*, *Ceratophyllum demerseum*, *Juglans regia*, *Fraxinum excelsior*, *Arundo phragmites* and *Equisetum palustre* (*op. cit.*, p. 12).

This evidence is quite consistent with itself and with that we have found in Britain. In all these cases the marls lie on sands or gravels belonging apparently either to a corresponding bed of crag or to the miocene beds of North Germany and are overlaid by boulder clay, and the champions of interglacial beds have been exercised to explain them away or largely to ignore them. Some of them have concentrated their attention upon one or two quite exceptional deposits as if they were the rule instead of being quite the exception. It will be well to examine one or two of these cases, and, first, that at Rixdorf.

The most important find of mammals of this kind made in Germany, and of which a great deal has been made as a proof of an interglacial age, is that at Rixdorf, where *Elephas primigenius* and *antiquus*, *Rhinoceros tichorinus* and *leptorhinus*, *Bos priscus Cervus Megaceros* and *Ovibos moschatus* have been found in a bed which is stated to be underlaid as well as overlaid by boulder clay (*Zeitschrift der Deutsch. Geol. Gesellschaft*, xxxi., p. 152). This deposit has always seemed to me to be a very

dubious one. The finding of *Elephas antiquus* in an interglacial bed is, it seems, almost as probable as finding an African antelope in such a bed. Again, in regard to the nature of the subjacent clay, shells of *Paludina* and of *Neritina fluvialis* have been found in it. This does not look like a boulder clay, nor are we told whether it was stratified. Thirdly, it seems from its original describer to be not like a continuous bed but an insertion, for its discoverer says of it "*welches sich nach innen zu etwas zu senken scheint*" (*ibid.*, xx., p. 648).

More lately Dames, who is a strong advocate of interglacial beds in Germany, and who formerly treated the Rixdorf deposit as lying between what the Germans generally call their upper and lower boulder clays, has seen reason to modify his views, and now puts it much lower. Thus he says of a corresponding bed close by: "Eine zuerst vom Verfasser in einem Bohrloch der Rixdorfer Vereinsbrauerei gefunden, in dem letzen Jahrzehnten durch immer zahlreichen Funde als fast unter ganz Berlin bis nach Cöpenich durchgehend erkannte Paludinenbank *als noch älter erwiesen*, so dass sie nur Interglacial zwischen der in Norddeutschland mit Sicherheit noch nicht nachgewiesenen ältesten oder ersten und der zweiten bisher als ältere bezeichneten Vereisung *oder gar als præ-glacial augesprochen werden muss.* Die schaalreste," he adds, "am den meisten uebrigen Punkten, und namentlich ueberall in dem Geschiebe Mergel, mussen dagegen als auf secondärer Lagerstätte betractet worden" (*Erlauterung zur Geol. Uebersicht der Umgegend von Berlin*, 1899, p. 45).

From Rixdorf we may turn to the beds at Klige, near Kottbus in South-East Brandenburg, from which forty species of plants have been described by Nehring and others, including *Pinus sylvestris*, *Picea excelsa*, *Betula verrucosa*, *B. odorata*, *Alnus*, species, *Salix aurita*, *S. repens*, *S. caprea*, *S. cinerea*, *Populus tremula*, *Corylus avellana*, *Carpinus betulus*, *Quercus*, species, *Tilia platyphylla*, *Acer campestre*, *Ilex aquifolium*, with some more marsh plants, etc. (Geikie, *op. cit.*, pp. 457, 458). This deposit has been widely advertised as an interglacial one. I do not know on what grounds. No doubt it is overlaid by a drift bed with boulders, which Dr. Keilhack describes as northern *Geschiebe Sand.* The other layers of clay, however, which are intercalated with it are described as *stoneless*, and are no more evidence

of true boulder clay or drift than the clay bed intercalated in the estuarine deposits of the Thames; while the bottom layer, upon which the notion of the bed being interglacial rests (if it rests on anything), is described as an arenaceous gravel, consisting partly of flints and other materials from the north and of *a large number of stones from the Sudetic Mountains*, *i.e.*, from the south. What claim can such a bed have to be a glacial deposit, or a deposit having anything in common with the northern drift?

This will suffice in regard to the evidences of subaerial and fresh-water remains in the drifts of North Germany. Their evidence, when closely examined, seems to me to furnish no foundation for the view that there was any intercalated mild period between periods of glacial age in Germany, and I am glad to be able to refer in support of the same view to the strong opinion of H. Credner, expressed at the International Congress of Geologists held at Washington in 1891.

If we go to countries lying to the east and north-east of Germany the evidence is unfortunately very scanty and very vague. Nikitin says he only knows of three cases of the mammoth having been found in the interior of Finland. "We have no information as to the beds in which they occurred. In the Baltic region of Russia and the Valdai Mountains the finds are very rare, and consist of single bones and teeth badly preserved in a doubtful or secondary position in the alluvions" (see *Méms. du Com. Géol. de Russie*, vol. i., p. 96).

In Central Russia Pander detected remains of mammoth and rhinoceros in reddish clay, covered by erratic blocks, eight versts to the south of Verschni Volstchok. In one spot, 300 versts south of St. Petersburg and twenty versts south of the river Kolomenta, he further found the horns of a stag in gravel or drift twenty-one feet below the surface and covered by fine yellow sand *which was surmounted by clay and northern erratic blocks* (*Russia and the Ural Mountains*, vol i., p. 65). Sir R. Murchison himself described the bones of the mammoth and woolly rhinoceros near Moscow as in reddish clay covered with erratic blocks. Belt speaks of having followed the northern drift southwards from Moscow and found it gradually change from clay with boulders to

the clay without boulders that covers the southern plains. "Around the Sea of Azof," he says, "cliffs of this glacial clay 100 feet high can be followed continuously for miles, and its junction below with the older beds is sharply defined. It rests on a fresh-water deposit, containing shells of species of *Unio*, *Cyclas* and *Paludina*, and at this horizon fragments of the tusks and bones of the mammoth are abundant, and are always undoubtedly older than the glacial clay. In a similar position the same remains have been found at Odessa and other places in South Russia" (*Quart. Journ. of Science*, vi., p. 290).

In regard to the famous skeleton of a mammoth found at Troizkoya, near Moscow, of which much has been made, Tscherski, a first-rate authority, says distinctly that it lay in a marine *pre-glacial bed.* He refers to the section given by Nikitin in the *Mémoires du Comité Géologique.* Tscherski, again, in describing the deposit of pleistocene animals discovered in 1878 in the government of Smolensk by Professor Dokutschayef, says it lay in a similar deposit. Its discoverer attributed it, apparently on *a priori* grounds, to a post-glacial age, but this view is contested and disposed of by Tscherski, who is supported by Nikitin (see the discussion in the *Mems. St. Peters. Acad.*, xl., p. 474). I do not doubt that, as in Germany and in England, numerous detached bones of the mammoth and its companions have occurred in the so-called glacial beds in Russia, being what we call organic boulders, but in regard to skeletons or bones in their undisturbed original beds I know of none which lie above the drift.

Turning to Scandinavia, the evidence for our purpose is very scanty indeed. I know of no instance whatever of subaerial or fresh-water beds in Norway intercalated between layers of drift or having any claims to be interglacial. In Sweden, peat with paleolithic implements and bones of the cave bear is described by Nillsen as underlying the Jära wall, a great ridge of sea gravel extending along the coast of the Baltic from Ystad to between Trelleborg and Falsterbo. "If this ridge be an asar," says Mr. Geikie, "as from the description may be inferred, and should it prove to belong to the great asar series, this would demonstrate that man had

inhabited Sweden before the last great submergence and period of floating ice" (*Geol. Mag.*, 1872, ix., pp. 259, 260). Erdmann tells us that a tusk of a mammoth was found seven feet deep in a bed of marl at Tittente, in Southern Scania, in a bed underlying the glacial clay (*Exposé des Formations Quaternaires de la Suède*, etc., p. 84).

In various parts of Scania we have remains of what Holst calls glacial fresh-water clays, which seem, according to him, to be contemporary with the Yoldia clays. Of these fresh-water deposits he says "*in gewissen Fallen moränenbedeckt sind*". L. Holmström found in one of these beds at Viminge, near Klögerup, species of *Pisidium* and *Limnæa*, and O. Torrell in the same place *Dryas octopetala*. In similar drift covered by what Holst calls *moränenbedeckten* clay, A. G. Nathorst found, in 1871 at Torsjo, *Limnæa, Pisidium* and *Cytheridea torosa*, as well as *Dryas octopetala* and *Salix polaris*. Nathorst considers this last deposit *remanié* since it is found on the top of a hill, but as to its actual horizon it is interesting to remember that it was under the drift and above the forest bed that the glaical plant bed described by Clement Reid and others occurred.

Holst also describes elsewhere beds of gravel containing *Anodonta* species, *Pisidium, Sphærium corneum, Limnæa ovata* and *Succinea pfeifferi* as overlaid by moraine matter (*unter morainer*). Nathorst claims to have found in 1875 at Klögerup two beds containing almost precisely the same biological débris separated by moraine stuff. In the bed above he found *Salix polaris, S. herbacea, S. reticulata, Betula nana, Dryas octopetala, Limnæa limosa, Pisidium, Anodonta* and *Cytheridea torosa*, and in that below, *Salix polaris, Dryas octopetala, Limnæa limosa, Pisidium, Anodonta* and *Cytheridea torosa*. How is it conceivable that on the same spot we should have an almost exact repetition of this kind separated by a geological period? It seems a great deal more likely either that the upper bed is *remanié* or has had the boulder bed driven underneath it, and that both beds with the plants and shells were contemporary. Holst says it is possible Nathorst was deceived, and adds: "Es ist auch moglich, dass die oben ausgesprochene Auffassung der Klägerupsbildungen in uebrigen in einen oder der andern Hinsicht als unrichtig befunden werden kann," and he con-

cludes: "*In Schonen finden sich also durchaus keine fossilführenden interglacialen Ablagerungen*" (*op. cit.*, pp. 13, 14).

These beds in Southern Sweden seem to me to be all of pleistocene age, and to date from before the deposition of the drift. They may be matched much further north in Sweden. Thus, at Frösö, in Jemteland, a bed of fossiliferous clay has been found containing seventy kinds of diatoms, fourteen kinds of moss, together with remains of the bracken fern. Although this bed is *overlaid* by five metres thick of so-called moraine matter, Munthe still raises the alternative that it may rather be interglacial than pre-glacial; on what ground I do not know (*ibid.*, pp. 35, 36).

Let us now turn to America. There, also, a long and persistent fight has taken place in regard to whether the mammoth is to be treated as a pre-glacial, an inter-glacial or a post-glacial animal. There also the ambiguity has arisen because the scattered bones of the great beast, as also trunks of trees and lumps of peat, have continually occurred, as we should expect them to occur, not *in situ*, but as transported and derivative boulders, and their true interpretation has therefore escaped their describers. This has been especially the case in Canada, where my old friend, Sir Wm. Dawson, whose views on the later beds were often sophisticated by other considerations than those of geology, very persistently attributed the mammoth remains to what he called post-glacial times. I have tried to analyse the evidence, and I can find no support for this view, but just the reverse. I may say that, with the American geologists, who have paid most attention to the question, I put the mammoth there on the same horizon as the so-called American forest bed and the fresh-water beds accompanying the latter.

Mr. R. Bell, who takes the same side as Sir Wm. Dawson, and writes with a good deal more positiveness than most of us think quite justified, has given an account of various remains of mammoth and mastodon found around Hudson Bay. In no instance is there any evidence as to the horizon whence the remains came. Thus a mammoth tooth was found in 1878 *on the rocky surface* of Long Island, which is thirty miles long, and lies off the Estmain coast, its south-west extremity being just north of Cape Jones, which is the point

where James Bay opens out into Hudson Bay proper (*Bull. Geol. Soc. Amer.*, ix., p. 370).

About 1896 an incomplete molar, which appeared to belong to El. Columbi, was found in the superficial deposits in one of the banks of the Saskatchewan river, about six miles above Edmonton, *but no particulars in reference to the discovery are available* (*ibid.*).

In 1877 a very perfect mastodon tooth was found *in the bed of the Moon river*, at its first bend below the junction of the Missinaibi and Mattagami. The jaw was found, but only the tooth was preserved by the Indians.

In 1853 some Indians found parts of the skeleton of a mastodon in the bottom of the valley of the Shell river, an affluent of the Assiniboine. Two scapulæ came to England, but nothing more definite is known about their horizon (*ibid.*, p. 383).

In the province of Quebec, according to Bell, no traces of fossil elephants have yet been discovered.

In the maritime province the only discovery yet made was of some mastodon bones, found in banks of sand and gravel in the valleys of Middle and Baddeck rivers in the central part of the island of Cape Breton. The two localities were not more than twenty miles apart, and in neither case were they more than fifty feet above the sea (*ibid.*, p. 390). Bell gives no hint about the horizon where they were found, nor does Dawson in his *Acadia* (3rd edit., p. 83); but the latter figures a bone and a tooth, both of which are rolled or weathered.

In the province of Ontario several discoveries of mammoth remains, etc., have taken place during the last seventy years, but unfortunately there is no exact account of their *provenance*. The following is a fair specimen of the notices that we find.

In regard to the remains of elephant found in 1852 at Burlington Heights, near Hamilton, at the west extremity of Lake Ontario, Mr. Billings says they were found forty feet below the surface and sixty feet above the level of the lake. The workmen who found them first pierced thirty feet of stratified and cemented coarse gravel, then through a layer of coarse sand, in which the bones were found. He adds, "*The*

geological age of this deposit is not yet determined with certainty" (*Geol. Surv. of Canada*, 1863, p. 24, etc.).

In a paper by Coleman in the *American Geologist* for 1894, p. 85, he describes the fossils reported at various times from the drifts of the Ontario province. He says that "in most recorded instances the exact geological horizon of the find has not been determined". The only cases quoted as of undoubted interglacial fossils are, first, one from Niagara Falls, where *Cyclas* was found in a sandy loam containing striated pebbles. The authority quoted is *Geol. Surv. of Canada*, 1863, p. 902. I will quote the passage as evidence of the kind of proof of "undoubted interglacial beds" accepted by some enquirers. Logan, in the volume in question, says: "At Niagara Falls the Silurian limestone is covered by 120 feet of sandy loam, holding striated pebbles and small boulders, and containing near the middle the shells of a species of *Cyclas*. It is overlaid by fifteen feet of thinly-bedded reddish-brown clay containing similar pebbles and angular fragments" !!!

The second instance is the well-known discovery by Dr. Hinde (see *Canadian Journal*, New Series, xv., p. 388, etc.) at Scarboro Heights, east of Toronto. Hinde found in these beds specimens of *Planorbis* and *Zonites*, the *elytra* of a *Carabid*, *Cypris* of two or three species, wood of pine or cedar, rushes and other plants, reeds, etc., some lycopods, mosses, algæ and diatomaceæ. *Over these beds* were large deposits of till, while the surface of the district was strewn with large boulders.

When we come to examine what there was below the plant bed, however, the evidence is most unsatisfactory. Hinde himself says: "At Scarboro these clays (*i.e.*, the clays with plants) extend beneath the lake level, and they might have been deemed of pre-glacial instead of interglacial age". The only reason he gives for making them pre-glacial is that in a small patch at Humber Bay, west of Toronto, where the beds have evidently been much disturbed, there seems to be a slight overlap between beds, which he identifies as the same as those at Scarboro. He says, in fact, "if it had not been for the small outlier of the fossiliferous clays at the Humber Bay the beds in the Scarboro cliff might have been regarded of glacial age". Upon this exceedingly fragile peg

the case for interglacial beds in Canada almost entirely hangs. To me the evidence is virtually worthless.

Mr. Coleman has described a more recent so-called interglacial bed in a cutting on the Belt line, in which many specimens of the following shells were found: *Pleurocera subulare, P. elevatum, P. pallidum* (?), *Valvata sincera, Sphærium striatnum, Unio phaseolus, U. clavus, U. pustulosus, var. schoolcrafti, U. occidens* (?), *U. luteolus, U. undulatus, U. rectus, U. trigonus, U. solidus.* Three pieces of wood were found in the same place, which have been provisionally assigned to *Fraxirus quadrangulata, Quercus obtusiloba* and *Taxus baccata,* var. *Canadensi.*

I will now give a section of the beds where this discovery was made:—

	Feet.
1. Sandy soil followed by brownish clay with boulders . .	3
2. Stratified bluish-grey clay (making buff bricks) . . .	69
3. Brownish or drab clay (making red bricks)	11
4. Brownish-yellow stratified sand	4
5. Brown sand with false bedding with thin layers of blue or brown clay	18
6. Blue clay (till with striated boulders)	3
7. Hudson River shales	30

The blue clay, number six, is described as indistinctly *stratified,* and as containing *Unios in situ* and specimens of wood. It seems to me to be a typical specimen of the Erie clay, and no more proves an interglacial bed than does the Erie clay in Ohio in a similar position, to which I shall turn presently. What is perfectly plain is that at this place the bed was overlaid by a great depth of drift beds (Coleman, *Amer. Geol.*, xiii.).

A much more definite and critical test case is furnished by an example from the Canadian seaboard.

Sir Wm. Dawson tells us he found in Nova Scotia no pleistocene shells, and the only evidence of organic life he found there during the boulder period, or immediately before it, was a hardened peaty bed, which appears under the boulder clay on the north-west arm of the river of Inhabitants in Cape Breton. It rests upon grey clay similar to that which underlies peat bogs, and is overlaid by nearly twenty feet of boulder clay. Pressure has rendered it nearly as hard as coal, though it is somewhat tougher and more

earthy than good coal. It contains many small roots and branches, apparently of coniferous trees allied to the spruces. The vegetable matter, he says, composing this bed must have flourished before the drift was spread over the province (*Acadian Geology*, 3rd edit., p. 63).

Let us now turn to the United States.

To this part of the American evidence I devoted a considerable space in my former work (*Glacial Nightmare*, pp. 475-478), where I have, I think, shown how extremely fragile is the foothold of those who claim to find any substantial evidence of the kind we are discussing in favour of an interglacial period. I will now only add a few additional paragraphs. The great deposits of bones contained in the so-called "salt licks" are not in an area occupied by the drift. Within the drift area most of the remains occur as sporadic bones, and they must be treated as erratics, just as the tree trunks and logs, which I deem to be contemporary with the mammoth. They have been frequently found there embedded in tough boulder clay. Before we consider the land surface evidenced by the mammoth remains, etc., a little more closely, I should like to try and find some reason for the fact that among the American geologists there should be such a divergence of opinion about the true horizon of this forest bed. The state geologists of Ohio hold that it is post-glacial or interglacial, while those of Illinois and Indiana hold it to be pre-glacial.

I venture to think that this is because the Ohio geologists have treated the basement blue clay which underlies the dirt or forest bed of that state as a glacial deposit or a drift deposit, whereas I believe it to be nothing of the kind. This blue clay has been treated as the equivalent of the so-called Erie clays by Prof. Newberry and others, and I do not quarrel with the identification; but I would remark that Sir Wm. Dawson says of the latter as it occurs in Canada, *towards the north these clays contain boulders and stones, but do not constitute a true boulder clay.*

They have afforded no fossils except drifted vegetable remains (*op. cit.*, p. 5). Again, the Erie clay differs entirely from the Leda clay, which is a true drift bed, in that the former burns into white bricks, while the latter burns into red ones (*ibid.*, pp. 23, 24).

The clay as it occurs in Ohio is described by Dr. Newberry as a tough blue or grey clay, thickly set with small pebbles or fragments of stone, and it also contains a few, usually small boulders. It is sometimes widely stratified throughout, and in many localities the upper portion is very finely and distinctly laminated and without pebbles (*The Surface Geology of Ohio*, p. 21).

This description is assuredly very different to that of the till or boulder clay we know in Europe, and to that widespread boulder clay in America which occurs at a higher level. It seems to me to belong to an entirely different horizon than the so-called glacial period, and to have nothing to do with it. If a few rolled foreign stones occur in it, so they do in the Red Crag, and I am not at all sure that it may not synchronize with that deposit.

On the other hand, the drift beds, which unmistakably overlie and in many cases cover the forest bed to an enormous depth, are undoubtedly true drift beds, characterised by true erratics, while, as Dawson says, on *their surface are scattered boulders and blocks of northern origin, often of great size, and in some cases transported two hundred miles from their original places.* These are, in fact, the equivalents of the European drift beds.

It is the beds which in so many places constitute the remains of what the Americans call the forest bed which, in my view, represent when intact the old land surface over which the drift was distributed, by whatever process it was so drifted. It was, further, the old land surface on which the mammoth and its companions lived, which, as we have shown from the European evidence, was pre-glacial and not post-glacial, in the sense in which these terms are used by the glacialists, and, in my own sense, it is simply older than the distribution of the drift. When we speak of this forest bed being older than the drift, or, to use the language of the glacialists, "pre-glacial," we do not mean that the forest bed is older than the so-called Erie clays of Ohio, but is older than the true drift, which, so far as we know, overlies it everywhere.

I will now take from Newberry's pamphlet a number of instances of the finding of this forest bed *in situ*.

1. Ross County, Ohio.—Wood, apparently cedar, from a

well in clay, thirty feet from the surface, 150 to 200 feet above Scioto river. (Col. Whittlesey.)

2. Coventry, Summit County, Ohio.—Muck and branches of trees, forty-two feet beneath the surface, in a well 544 feet above Lake Erie. (Col. Whittlesey.)

3. Cleveland, Ohio.—A carbonaceous stratum, with many trunks of coniferous wood on surface of Erie clay beneath twenty feet of sand and gravel and clay, fifty feet above Lake Erie.

4. Hamilton County, Ohio.—Thirty-five wells, containing muck beds, leaves or timber, from 300 to 500 feet above the Ohio. (Col. Whittlesey.)

5. Oxford, Butler County, Ohio.—An upright trunk and roots of a tree in blue clay, at a depth of thirty feet. (D. Christy.)

6. Highland County, Ohio. In the village of Marshall eleven wells out of twenty have reached a stratum of vegetable matter, with leaves, branches, roots and tree trunks. Many similar cases in the same county. (Orton.)

7. Clermont and several adjoining counties.—Ancient soil above the boulder clay (? the Erie clay, H.H.H.) and below the upper drift deposits. (Orton.)

8. Germantown, Montgomery County, Ohio.—Bed of peat twelve to twenty feet in thickness, the surface covered with sphagnous mosses, grasses and sedges, and containing quantities of coniferous wood, with twigs, branches and berries of red cedar; *also containing bones of elephant and mastodon and teeth of giant beaver*, the whole covered with ninety feet of gravel and sand. (Orton.)

9. All through South-West Indiana ancient soil, with peat, muck, rooted stumps, trunks, branches and leaves of trees, two to twenty feet in thickness, sixty to 120 feet below the surface, called "Noah's cattle yards". (J. Collett.)

10. Peoria County, Illinois.—Drift over coal measures; average thickness seventy feet, consisting of blue clay below fifty feet thick, overlaid by old soil, with cedar timber; above this, yellow clay and sand sixteen to twenty feet thick; section shown by thirty-nine borings and many wells. (W. Chapman.)

11. Lawrenceberg, Indiana, and many places in Ohio valley. —Old soil with trunks and roots of trees, the latter *in situ*,

layers of leaves, ripened fruits, grasses and sedges, all clearly distinguishable. Several of the species of trees and plants can be determined, some by their wood, others by their leaves and fruit. Among them may be named the sycamore, beech, shell-bark hickory, buckeye, red cedar and wild balsam apple, six feet above low water-mark and forty feet below flood plain. (Orton, vol. i., part i., p. 427.)

12. Several counties in Iowa.—An old soil with buried timber, from forty to fifty feet below the surface, struck in sinking wells over several counties. (Morris Miller, in letter.)

13. Walworth County, Wisconsin.—Timber resembling white cedar, from a well eighteen feet deep in the prairie region, about 250 feet above Lake Michigan. (J. A. Lapham.)

14. Appleton, Wisconsin.—Red cedar in red clay, eighteen feet below the surface, 150 feet above Lake Michigan; also white cedar, thirty feet below the surface in red clay. (Dr. E. C. S. E. Beach, cited by Col. Whittlesey.)

15. Green Bay, Wisconsin.—Apparently willow in red clay, fifty feet below the surface of Lake Michigan. (Col. Whittlesey.)

16. Iowa County, Iowa.—Two logs of resinous timber in a well sixty feet deep on general level of country. (Col. Whittlesey.)

17. Grand Sable, south shore of Lake Superior.—Layer of roots and limbs of trees, sometimes twelve or fourteen feet thick, resting on bluish-drab clay, covered with sand interstratified with gravel, 300 feet thick. (Sir Wm. Logan, *Geology of Canada*, 1863, p. 905.)

18. Toronto, Canada.—Trunks and branches of trees embedded in yellow clay overlying blue clay, at a depth of from ten to twenty feet above the surface. (Prof. Hinde.)

Inter alia, Newberry argues from these cases (1) that a forest occupied the surface long enough to produce a deep carbonaceous soil over all the lower and more moist portions, (2) that in the marshy portions of this land surface beds of peat were formed, in some instances even twenty feet in thickness, and (3) that most of the ancient forest was coniferous and cedar and cranberry grew in the bogs; from which we may infer that the climate was colder than now in the same region. In the forest bed we find the remains of the mammoth,

mastodon, giant beaver and some other animals. In regard to the great beaver, Newberry says: "In several of the wells which penetrate the forest bed, chopped timber and chips are reported to have been found. As the number of such cases is so numerous, we must suppose that the stories are founded on fact, and I have suggested that possibly the chopping was done by the great dental chisels of the giant beaver" (*ibid.*, pp. 30-32).

All this seems very sound, and the only point in which I differ from Professor Newberry, and which I have excluded from this summary, is in his making the blue Erie clay an older deposit instead of part of the so-called glacial series. To that series I attribute the yellow clay with boulders covering the forest bed, and also the enormous and numerous erratics which are found on the surface, a list of the most remarkable of which as well as of their mode of occurrence is given by Newberry in the work just cited (*op. cit.*, pp. 38, 39).

It is doubtless in the sense in which I write that some very good authorities in America have persistently maintained that the forest bed underlies and does not overlie the drift. Thus Prof. Wright, in referring to the discovery by Prof. Orton in 1870 of the forest bed at Germantown (see above, number 8), speaks of the ninety feet of deposit overlying it as being till, and he adds that both the peat and clayey till above it contain many fragments of coniferous wood, which can be identified as red cedar (*Juniperus virginiacum*); and he continues: "In numerous other places in that portion of Ohio fresh-appearing logs, branches and twigs of wood are found underneath the till or mingled with it, much as boulders are. Near Butler County, Ohio, red cedar logs were found under a covering of sixty-five feet of till, and so fresh that the perfume of the wood is as fresh as ever."

Farther to the north-west similar phenomena occur. Prof. N. H. Winchell has described them most particularly in Fillmore and Morven Counties (Minnesota), from which they extend through a considerable portion of Iowa. In the above counties of Minnesota a stratum of peat from eighteen inches to six or eight feet in thickness, with marsh and sod, is pretty uniformly encountered in digging wells, the depth varying from twenty to fifty feet.

At Barnesville, in Clay County, Minnesota, which lies in the valley of the Red River of the North, and also in Wilken County, in the same valley, tarn or rock wood and sandy black mud containing many snail shells (*helices*) have been found from eight to twelve feet below a surface of till.

That the forest bed in Iowa is overlaid by till is quite certain. This is admitted by those who also believe in an underlying till. Thus Mr. Calvin says: "Remains of a forest bed which was overwhelmed and buried by advancing glaciers are conspicuous in many of the drift sections in North-East Iowa. The abraded and splintered wood is distributed through a zone a number of feet thick, but it is most abundant in connection with or just a little above a definitely marked soil, bone and peat horizon. The principal belt through which forest material is distributed lies above the soil and peat" (*Bull. Geol. Soc. Amer.*, x., p. 113).

Prof. Todd describes finding a bed of marsh and soil under twenty feet of silt and clay near Grand View in South Dakota, and he adds: "I have frequently found traces of wood in till in Dakota, but always in an isolated way. I think, from reading statements about the deposits in East Iowa, that most, if not all, of the cases are of this sort." He refers especially to the mistakes of well-borers, in whose eyes a log easily becomes a forest bed. "Prof. Worthen insisted that there were no *interglacial* soils or forest beds in Illinois. He was very confident in referring most of them to pre-glacial times" (Wright, *Man and the Glacial Period*, pp. 107-112). This is what he says in his last publication as head of the Geological Survey of Illinois.

"Trunks of large trees are sometimes met with in sinking wells through the boulder clay, but no animal remains have been authentically reported from it except certain cretaceous fossils. The boulder clay is frequently *underlaid* by a black, peaty soil, varying in thickness from two to thirteen feet, filled in many places with twigs, branches, occasionally whole trunks of trees, the wood in many cases being in a good state of preservation" (*Geol. Surv. of Illinois*, viii., p. 9).

Prof. Udden says: "In the region south of the Wisconsin driftless area an old soil is occasionally found under the Kansan drift, generally resting on bed rock and often as-

sociated with laminated water-bedded clay and other silt. An exposure of such a bed occurs under a bluff of drift in the south part of Muscatine. . . . It lies below what appears to be Kansan drift. A similar bed was uncovered on the east side of Eastern Avenue, at Davenport, Iowa. The same bed has been encountered at Rock Hill, Illinois, in several wells. In one of the latter were found gasteropods; below it was a sticky clay with fragments of bed rock, but apparently no archæan pebbles or boulders. This clay rested on coal measures, and seemed to be residual clay of pre-glacial age. The silt and muck above it contained fragments of wood, one of which measured nearly two feet in length. In another well in Thirty-Ninth Street, at Rock Island, Illinois, *Helicina occulta, Pupa atticola, Pyramidula striatella* and *Succinea avara* were found. Similar deposits without fossils occur under drift on the bluffs east of Cordova, Illinois, and in the north part of Chistor, Iowa. At the latter place they are laminated and associated with a peaty or soil-like layer. All the fragments of wood found in the ancient soils belong to gymnosperms. . . . The position of the deposits under the till indicates that they are pre-Kansan in age and probably pre-glacial" (*American Geologist*, 1898, pp. 263, 264).

What is true of the forest bed is true also of the very few cases where the remains of the great beasts have been found in the United States not as boulders, but *in situ*. These cases are none of them conclusive, and I know of no actual juxtaposition of boulder drift and intact skeletons in America at all, but they seem to point the same moral exactly as the European instances. In 1838 an almost complete skeleton of an elephant was found in Jackson County, Ohio. It was found *under a mass of stratified material* fifteen to eighteen feet in thickness (Billings, *op. cit.*, p. 32).

In 1852 the skeleton of an enormous elephant was found at Louisville, Ohio, and described by Prof. Wyman in the *Proceedings of the American Association* (1857). It was found in a stratum of sand which was *overlaid by some large blocks evidently transported from the neighbouring carboniferous* and Devonian formations (Billings, *Elephant Remains in Canada*, p. 34).

In the *Report on the Geology of Ohio*, 1878, vol. iii., p. 593,

are several notices of the discovery of mastodon bones in Coshocton County. Nothing is given to show the horizon, but in one case some large bones are said to have been found in a bed of blue marl, forty-two feet deep, in digging a well. These were probably *in situ*. The others were apparently sporadic bones. Warren in his *Memoir on the Mastodon*, pp. 155-6, says that Mr. Foster, who had made a great many observations of the Mastodon localities, had assured him in the strongest terms that he had repeatedly seen collections of their bones *below* the drift and boulder deposits.

In summing up these facts it seems to me to be established as very probable that the horizon of the mammoth and that of the American forest beds is pre-glacial. Teeth and bones no doubt sometimes occur in drift as boulders, but when they are found *in situ* they underlie the drift and do not lie upon it, nor are they intercalated with it. So much for the evidence of the land fauna and flora. Let us now turn to the marine débris associated with or contained in the drift which have been supposed to attest to oscillations of climate.

I propose to limit my present survey to the mollusca and other higher forms of marine life. The *foraminifera*, whose evidence is so important, and has caused so much heart-burning among some glacial geologists of late, I shall discuss in a later chapter, when we deal with the origin of the so-called glacial clays.

I shall begin what I have to say on the marine débris in question with a short survey of the evidence from Britain, where a great change has come over the opinions of geologists since Searles Wood, Jun., and Harmer professed to find evidence of interglacial climates in the mollusca of the drift beds of East Anglia.

Speaking of these drift beds in East Anglia, Mr. H. B. Woodward says: "These sands and gravels form portions of the higher ground near Cromer, and they are well exhibited in the cliffs near Yarmouth and Lowestoft. The sands in places contain shoals of broken shells, and these, more conspicuous in the cliffs at Gorleston, Hopton and Corton, between Yarmouth and Lowestoft, *were noticed by the earlier geologists as re-deposited crag shells.* These shells were found at Caistor, near Yarmouth, in 1836 by Mr. John Gunn, and they have also

been found at Billockby in the same neighbourhood. Messrs. Wood and Harmer have procured upwards of one hundred species of mollusca from these sands, and the assemblage is a very curious one. With the exception of one *Venus*, a *Loripes* and some new species, all the shells are found in our crag deposits; but while including a number of species which might have come out of the Coralline, Red and Norwich Crags, yet "not a trace or a fragment of most of the common strong shells of the Coralline and Red Crags has occurred". Nearly all the specimens found are more or less rolled, but fragile shells like *Anomia ephippium* are occasionally preserved. Messrs. Wood and Harmer concluded that the fauna was contemporaneous, although the shells bear evidence of having been shifted and rolled by currents which brought them from some other part of the sea bottom; while in their opinion some of the delicate shells may have been transported by floating masses such as sea-weed. These conclusions, I believe, says Mr. Woodward, first started the notion of interglacial mild periods,[1] for the presence of Coralline Crag forms suggested that some connection with the Mediterranean area had been reopened by submergence of the land at this period (*Proceedings of the Geol. Assoc.*, ix., pp. 2, 3).

The views of Messrs. Wood and Harmer were followed by Clement Reid in his *Geology of the Country round Cromer*. The position was traversed, however, by Horace Woodward. He says: "I have for some time felt great doubt about the shell fauna having been contemporaneous. Of course the aspect of the shells alone makes one sceptical, and it is admitted they did not live on the spots where they have been accumulated. These gravels," he says, "pass southwards into gravels which underlie the chalky boulder clay"; and he further says of them that "they do not indicate deeper water conditions; nor have we any southerly fossiliferous beds of the age which we might expect had the fauna migrated in interglacial times from the Mediterranean area," and he suggests that the middle glacial shells may have been largely derived from old crag accumulations, now entirely destroyed or buried beneath the waters of the North Sea.

[1] I believe Croll really first suggested them.

Speaking, again, of the shells found in the so-called glacial sands of the Cromer cliffs, Woodward continues: "There is nevertheless room for great doubt about the shell fauna being contemporaneous. Traced southward, these so-called middle glacial sands pass mostly into gravels which underlie the chalky boulder clay over a considerable part of Essex, and only in two or three places in Suffolk, south of Corton, have fragments of crag shells been found in the drift of this portion of the eastern counties." He further observes that the *ostracoda* from the sands at Hopton Cliff, near Yarmouth, instead of evidencing a mild interglacial climate, present "a generally arctic climate" (*Geol. of England and Wales*, pp. 504, 505).

Mr. Clement Reid, who formerly held a different view, has for many years been in agreement with Mr. Woodward on this point. Thus, in a letter I received many years ago from him, he says: "The fauna of the middle glacial sands of Norfolk I now have no doubt is entirely derivative. Formerly I accepted Searles Wood's view as to the contemporaneous age of the mollusca, because of the apparent force of the argument from the perfect preservation of thin and delicate shells. The argument I now see has no value, for shaken about *under water* thin shells are less injured than thick ones. The extraordinary mixture of species belonging to different sea bottoms, zones of depth and climates is also against the acceptance of the fauna as a homogeneous one."

What is true of the so-called middle sands of East Anglia is equally true of the shingle beds which occur there. They also contain shells in certain places. These shells have not been found in the corresponding beds inland, and only occur in certain localities in the eastern parts of East Anglia. This everyone is agreed about. As Prof. Prestwich says, "they are found only in that part of the beds occurring on the seaboard of the eastern counties". They occur very prominently at Southwold, Henham and in the Bure valley. The shells in these beds are apparently all derivative and have been derived from the crag. They are either contained in pockets of crag sand or are in close contact with crag beds, and, as is well known, the species of shells found in them are all crag shells. Mr. Whitaker has described twenty-four species from

this shingle in his excellent memoir on the Southwold shingle, every one of which is derived from the crag. Similarly, in the Bure valley the shells found in the sands which occur with the shingle are identical with those in the Weybourn sands, another crag deposit. This view, which I have long held (*Geol. Mag.*, 1895, p. 495), was hinted at by Prof. Prestwich. Although he did not adopt it positively, he says, speaking of the shells in the shingle, "I am not quite satisfied that these shells, or at least all of them, belong to the Westleton beds". At Henham, casts and impressions of shells were found in an iron-concreted portion of the shingle, while at the bottom of the pit actual shells were found. At Southwold the shells occurred in a lenticular mass six inches thick and about five feet deep in a cutting seven feet deep and in another small patch about twelve yards northwards, and it seems to me they have been derived from the upper crag (the Chillesford sands). Mr. H. Woodward, while making the Bure valley beds another horizon, treats them as part of the Norwich Crag series. I am therefore of opinion that the shells which are found in certain limited localities only in the shingle beds, generally at their base and in close contact with Crag deposits, are in every case derived or *remanié* just as the shells in the Middle sands are, and that they in no wise mark the real horizon of the shingles.

The marine shells found in various places round the Fen country are, I believe, also pre-glacial, although here we have no direct evidence of their being either covered or underlaid by drift beds. In the valley of the Nar Mr. Rose described several of these deposits (*Geol. Mag.*, ii., p. 8), all of them clearly *remanié*, since marine shells were found in the same clays with the remains of the mammoth, the rhinoceros, the horse and red stag. In the only section given by Mr. Rose the shell bed lies directly on a blackish sand of the lower greensand.

I cannot agree with Mr. H. Woodward that some of the shells described by Mr. Rose are estuarine shells, notably the large oyster shells, etc. Mr. Woodward has given a very small selection from what is really a significant list (see *Geol. Mag.*, ii., p. 11). The shells have no doubt come from different depths. The shell beds at March and in the other gravels

of the Fens, which agree so closely in contents with the East Anglian drifts, and seem to be associated with the mammoth-bearing beds, seem also to be explainable only in the same way, namely, that they are the remains of pre-glacial beds.

Sedgwick, Searles Wood and Harmer speak of the position of these gravels as doubtful, and of our having no means of testing their position in regard to the drift. Indications of their dipping below the boulder clay have been mentioned, and Seeley describes them in one place as overlaid by three feet of brown clay which he seems to have thought boulder clay. Mr. H. Woodward says they are probably of the same age as the marine gravels at Kelsey Hill, and he admits they are possibly older than the basement clay (*Geol. of England and Wales*, p. 500).

In regard to the Yorkshire shell beds, Mr. Searles Wood, Junr., in 1882, while correlating the Yorkshire basement clay with the Cromer till, still argued that the shell bed at Bridlington was in place and that the molluscs lived where they are now found (see *Geol. Mag.*, 1882, p. 192). The bed in question was in fact described as a seam or bed of shelly sand (in place) in the purple boulder clay. About 1882 Mr. Lamplugh tells us he was able to show that neither these shells nor those similarly found at Dimlington, near Spurn Point, had been obtained from beds in place, but from masses of sand and clay occurring as boulders in the basement boulder clay. In 1884 he read a paper on these beds before the Geological Society, in which he tells us that the fauna in the transported patches varied greatly; some contained many shells throughout, others very few, occasionally none; others had shells in one part but not in another, and one mass seemed to be of fresh-water origin, as it consisted of very fine unctuous clay without stones or shells, through which ran a thin seam of peaty matter, in which traces of moss, wood and the seeds of *Potamogeton* were detected; most of the shelly patches contained *Foraminifera;* the great majority of the shells were crushed and broken and the fragments scattered; there were also fish and otherremains; uninjured shells sometimes occurred. Mr. Lamplugh says he found *one* specimen, and one only, of *Astarte compressa*, with valves united, and one perfect *Tellina baltica*, while in the basement clay itself he once found a *Tellina*, with valves closed

and filled in with sand. Though he says he himself only found some unbroken specimens of the stronger bivalves, such as *Astarte*, *Mya truncata*, *Pholas crispata*, *Saxicara rugosa*, etc., Mr. Headly, by his sifting process, brought to light magnificent specimens of *Nucula cobboldiæ*, *Tellina calcarea*, several species of *Leda*, as well as a large number of the smaller shells—especially univalves, which have escaped injury far oftener than the larger individuals. Mr. Lamplugh urges that while the beds are not in place the greater part had a common origin, and they are all of one age. In the discussion which followed the reading of this paper, Dr. J. Gwyn Jefferies said he believed from personal inspection that this was a *remanié* deposit. The shells were much more arctic in character than those at Moel Tryfaen. In his reply Mr. Lamplugh said, *inter alia*, that the difference of the sand forming the patches from that formed from the waste of the rocks on the coast was an argument against their having come from the neighbourhood (*Quart. Journ. Geol. Soc.*, 1882, p. 328).

In 1885 Mr. Clement Reid, in his *Geology of Holderness*, fully admitted the fragmental and transported character of the shell beds. Mr. Lamplugh remarks on the fact that the basement clay contains patches of other deposits containing shells, etc. These inclusions he says are not always beds of glacial age, but are sometimes derived from the secondary formations, masses of Liassic shale and Neocomean clay, the former occurring, among other places, in Filey Bay and at Bridlington, and the latter at several places on Flamborough Head. With such distinct proofs of transportation from a distance in some cases, we can scarcely resist the conclusion that in others also, though the evidence is less decisive, the beds are likewise far from their original positions, especially in the case of the shelly patches, which possess characters showing that they can hardly have been formed in the neighbourhood in which we find them. Speaking of a particular patch of the shelly sand, Mr. Lamplugh says: "It is most instructive to find that in this, the only instance in which we have been able to study the shell-bearing patches in full section, and to see the base of the drifts below, that though there are stratified beds lower in the series these are quite different in character from the fossiliferous sand, contain no

fossils, and are probably not of marine origin, thus lending no countenance to the view that the shelly patches have been derived from beds immediately underlying the boulder clay. . . . Nothing could be clearer than that the scrapings of a sea bottom have been incorporated in the basement clay. . . . Many of the shells indicate comparatively deep water, and may have come from almost anywhere in the bed of the southern part of the North Sea." Mr. Lamplugh argues strongly that the basement clay is the oldest so-called glacial deposit in England, and that when the shells were moved (of course he thinks it was ice that moved them, which I think impossible) it was from a floor of bare secondary rocks, save when they carried a thin covering of marine beds, which were practically contemporaneous; for there are strong reasons for thinking that the shells of the fossiliferous rocks were alive very shortly before their transportation. On both sides of Flamborough Head, at Sewerby and at Speeton, we find the relics of marine beds below the basement clay and its associated chalk rubble, that appears to have accumulated before there was any boulder clay existing in the neighbourhood; and these prove an open sea, a shore line and a land surface tenanted by numerous large mammals, such as the elephant, rhinoceros, hippopotamus, bison, etc., at a period not long anterior to the formation of the basement clay, the relative level of sand and sea not being very different from what we find at present (*op. cit.*, pp. 276-292).

In regard to the fish remains from Bridlington, Mr. Newton says that nearly the whole of them are either Norwich Crag, Red Crag or London Clay forms, and, seeing that so many of the Crag vertebrata have been originally derived from the London Clay, it is quite possible that all the Bridlington fishes have been derived directly from the crags. "I should doubt if any of them were contemporaneous with the Bridlington deposits, and the mineral condition and polished surface of the specimens are characteristic of Crag fossils. This would seem to point to the destruction of older tertiary beds during the formation of the gravelly sand containing the arctic fauna" (*Quart. Journ. Geol. Soc.*, 1884, p. 322).

In my former work (*Glacial Nightmare*, p. 471) I quoted some of Mr. Lamplugh's general conclusions in regard to

interglacial beds in Yorkshire. His latest statement on the subject was read before the Hull Geological Society in March, 1898. In this he says: "My colleague, Mr. Clement Reid, whose opinion on such subjects is entitled to the greatest respect, after surveying the greater part of Holderness came to the conclusion, which I believe he still holds, that the gravels with fragmentary marine shells which occur as mounds and ridges in various parts of Holderness are of marine origin and denote a mild interglacial stage, and may be equivalent to the buried cliffs of Sewerby and Hessle. From this," he says, "I have always felt compelled to demur, and I doubt the correlation of the beds. On what he bases his doubts I do not know, for he says that he considers the presence of interglacial beds in the district to be still unproved" (*op. cit.*, p. 6).

C. Lewis says that "the character of the shells in the Weybourne Crag accords well with that of the shells in the pre-glacial beds at Speeton. No interglacial beds are known on the Holderness coast" (*Glacial Geology*, p. 367).

Extremely comminuted shells have recently been found in the drift beds of Durham and of Berwickshire. Their condition proves their character. Let us now travel northwards.

In 1860 Sir R. Murchison communicated to the Royal Society a notice by Mr. T. F. Jamieson of his having found in the parishes of Slains and Cruden in Aberdeenshire certain beds of stratified sand and gravel *beneath boulder clay*, and in which neither polish nor striæ could be found on the pebbles, and which he assigned to the Red Crag. He especially claims to have found *Cyprina rustica*, not found in the drift and only in the Crag, also *Fusus antiquus*, var. *contrarius*, another Crag form (*Geol. Proceedings*, 1860, p. 371).

In regard to these beds Mr. H. B. Woodward says: "I mention these facts to show that older pliocene as well as newer pliocene accumulations took place so far north as Aberdeenshire; and hence all difficulty is removed in seeking a northern derivation for the shell accumulations in the so-called 'middle glacial' sands of East Anglia. . . . Hence we are justified in regarding the question at any rate as an open one and in making the suggestion that the middle

glacial shells may have been largely derived from old Crag accumulations, now entirely destroyed or buried up beneath the waters of the North Sea" (*ibid.*, pp. 4, 5).

Describing the shell bed at Nairn, at an elevation of 500 feet, C. Lewis says: "Dr. Crosskey has recently visited this place, and confirms Mr. Fraser's description of it. An undisturbed mass of clay occurs there, full of unbroken delicate marine shells, with united valves; over it, separated by a sharp line, is a sand bed, *and over this, unconformable to it, and sharply separated from it, is a large mass of stony till.* This old sea bed has been protected from glacial erosion by surrounding crags of rocks. No erratics are known in this sea bed, which may therefore be pre-glacial. It is an old estuary, perhaps of the similar age to the pre-glacial shell bed of Speeton (100 feet high) and the pre-glacial marine and estuarine beds of the Norfolk coast. There is no evidence that it is interglacial, or that the submergence it indicates was not anterior to the elevation which brought on glaciation" (Lewis, *Glacial Geology*, pp. 375, 376).

A committee of the British Association reported upon these beds, and I cannot sum up their report better than in Prof. James Geikie's own words. He says: "The majority of the committee, M. J. Horne, F. T. Jamieson, David Robertson and J. Fraser, are of opinion that the shell bed is *in situ*, that the marine organisms lived and died where their shells are found, and that *subsequently the region was overflowed by glacier ice which deposited the superadjacent boulder clay*" (*Great Ice Age*, pp. 140, 141).

Mr. Horne, of the Scotch Survey, says that the high level shelly tills of Nairn, Elgin and Banff "are found to be overlapped by a reddish-brown boulder clay of the normal unfossiliferous character".

Mr. Geikie devotes many paragraphs to discussing what he calls the shelly till of the far north of Scotland, and which he graphically describes as "containing broken, crushed and striated shells which are scattered confusedly through the mass much in the same way as the stones and boulders with which they are associated" (*ibid.*, p. 137). That is to say the shells in question are derivative and not at home in the beds, and belong in fact to a previous horizon. This is the case

with the shelly till covering a large part of Caithness. Mr. James Geikie again speaks of these derivative shells as "broken shells and other marine organisms," many of the shell fragments showing fine glacial striæ. It is not common, he says, to meet with a perfect specimen, but several shells have been found with both valves complete, and more or less well preserved single valves are not very rare. In general, however, they are mere fragments, and are scattered through the clay in the same irregular manner as the stones and boulders. It is noteworthy that they show a curious mixture of arctic, boreal and southern forms. Among them only a small number are characteristic of the littoral zone, the most abundant being species whose habitat lay in deeper water. It may be added that the shells occur most plentifully along the eastern seaboard of Caithness, being gradually scarcer as the till is followed inland towards Thurso and Reay.

Messrs. Peach and Horne have recently shown that a similar shelly boulder clay occurs in the Orkney Islands. The numerous shell fragments are often smoothed and striated in the same manner as the glaciated stones with which they are associated (*ibid.*, p. 139).

Having discussed the shelly deposits on the borders of the North Sea, let us now turn to those on the western side of Britain. A word only about the remains of shells which have occurred in Shropshire and in the watershed of the Severn. These are associated with the remains of the mammoth and are very broken and disintegrated. They seem to me to be most distinctly derivative, and not to evidence, as Murchison thought, a former arm of the sea existing in Salopia.

Mr. M. Reade, in the *Quarterly Journal of the Geological Society*, 1874, p. 27, in describing the drift beds of North-West England, says that the shells are much rolled, and that they could not possibly have lived together on the same bottom, some being peculiar to sand, others to mud; some to rock, others to shingle; some requiring deep water, and others shallow; so that the conclusion is irresistibly forced upon us that they must have been to a large extent transported. These shells occur at Liverpool, Blackpool, Warrington, Chester, etc., and with the exception of three species may

be described as Irish Sea shells, but as a rule they have more northern facies than those now living in the Irish Sea.

My friend Mr. R. D. Darbishire described in the *Quarterly Journal of the Geological Society* for 1874, p. 38, a hill of gravel near Leyland, in Lancashire, which contained rolled fragments of shells. He thinks the shells did not live where they are found.

In the *Quarterly Journal of the Geological Society*, 1878, p. 383, Mr. Thom described rolled shells sorted by water according to weight as occurring about Newton-le-Willows. They are not *in situ*, and the valves of the bivalves are never united.

Edward Forbes argued long before that the shells in the north-western drift do not indicate a sea bottom or sea beach but have been transported from lower levels. The fauna of the upper clay at Newton, says Lewis, is clearly littoral or sub-littoral. The shells are not *in situ*. He says that the Liverpool boulder clay and its mollusca, described by Reade, is identical with this. The few southern shells in the Liverpool and Newton boulder clays are derived from the older sands (*Glacial Geology*, etc., pp. 361, 362).

The pleistocene shells collected by Dr. Crosskey in this district were of two different kinds; one of these classes may be described as "shell pebbles," since they are all transported and waterworn fragments. They are white, fragmentary and almost always rolled and rounded. They lie in drift sand, gravel or till, and are to be classed with pebbles. They do not lie in the position in which they lived. They occur at all heights up to 1,500 feet, occurring as high as any drift gravels occur. These are the shells of Moel Tryfaen, of Macclesfield, of Blackpool and of Loch Lomond. At Moel Tryfaen and at Macclesfield (perhaps also at the other points) they lie in rounded gravel and sand among the pebbles and have been transported from sea-ward. They are associated at Moel Tryfaen with flints from Antrim (Lewis, *Glacial Geology of Great Britain*, pp. 379, 380).

Mr. Kendall says: "The fauna of the middle sands and gravels yields no support to the theory of an interglacial period on the western side of England. Indeed, if reliance is to be placed upon the shells, there is ample justification

for the statement that the sandy deposits which have been called interglacial were laid down under conditions of greater, rather than less, severity than the boulder clays with which they are associated, inasmuch as they contain a fauna which, in its entirety, is decidedly of a more boreal type than that found in the boulder clays" (*Glacial Geology of Great Britain*, etc., pp. 411-413).

Mr. Hull, as we have seen, divided the so-called glacial beds of North-West England into three horizons, which were supposed to mark three periods—two glacial periods separated by an interglacial one. I have already criticised the view at some length (*Glacial Nightmare*, p. 849, etc.). Mr. Kendall has a pertinent paragraph in regard to it. He says: "No paleontological division can be established between the beds. I have revised the lists of the shells found in the West Anglian deposits by Mr. Shone, and added the lists Miss Ffarington has published of those found in her pit at Leyland, and find that the distribution of boreal, now British species, is as follows: lower boulder clay, 5; middle sands, 13; and upper boulder clay, 7. The distribution entirely disposes of 'the middle sands' as a paleontological division, at least in West Anglia" (*ibid.*, p. 426).

"In regard to the temperature," says Mr. Kendall, "the fauna is decidedly 'inconsistent' in every sense in which such a term could be applied. Along with a considerable number of shells of northerly range there are in every division of the series shells which fail to find a suitable habitat in latitudes so far north as Britain at the present day. *Cythere chione* does not now range to the north of Carnarvon Bay, where it is very rare, but it is found commonly in the three divisions of the Lancashire glacials" (*Glacial Geology of Great Britain*, p. 427).

Dr. Gwyn Jefferies says of these shells that the mixture of the northern and southern shells is very noticeable, and would seem to indicate that some of the beds were *remanié*. *Venus chione* and *Arca lactea* are peculiarly southern forms, and yet they occur with others which are only found in glacial deposits. Of the latter those found in Cheshire are of Scandinavian and not truly arctic type (*Quart. Journ. Geol. Soc.*, xxxiv., p. 395).

The shells, again, are not with their proper surroundings. "In sandy deposits there are clay-boring species, and in clays we find shells that are never found living in such a bottom. Strictly, littoral, rock-haunting shells, such as *Purpura lapillus* and *Littorina littorea*, are found in thick clays along with the comparatively deep-water *Saxicava norvegica* and *Tapes virginus*. . . . *The condition* of the West Anglian shells, no less than their association, is directly against the supposition that they are *in situ*. Through all this region I do not know an instance of a pair of valves found in apposition except in the bored stones at Leyland, and whole shells are extremely rare" (Kendall, *op. cit.*, p. 428). "I have searched the literature of the subject and failed to discover a single case, in the area to which I have limited myself more particularly, of sea shells in a deposit unaccompanied by . . . transmarine drift, that is, drift containing stones which must have crossed some part of the sea to reach their present positions." Kendall then goes on to say that he had examined three sites which were supposed to be exceptions to this rule, namely, one at Burnley in Lancashire, one at Gloppa near Oswestry, and a third at Halkin in Flintshire, and in every case had found them with transmarine rocks. In regard to the shelly drifts of the Isle of Man, Mr. Kendall says, "the condition of the shells is strongly opposed to the idea of their being in their native habitat. Throughout the sands and gravels the shells are, generally speaking, very much worn and broken, and I have not seen a single example of a bivalve having the valves in apposition."

"In the boulder clay met with in the sewer cutting in the Mooragh (a dark, tough clay full of stones) Dr. Tillet and Mr. Kermode found two examples of *Tellina baltica* with the valves united, but the remarkable fact is recorded by Dr. Tillet that though they were taken out of hard, grey-brown boulder clay, the valves were full of a white siliceous sand in which . . . I detected some *foraminifera*. Similar facts have been noted by Mr. Lamplugh and Mr. Shone, and the interpretation is plain: the shells lay in a sandy bed on the floor of the sea. . . . I incline to the opinion that the clay in which these shells occur is a portion of the sea bottom which has been transported bodily. . . . The nature of the

shells in the drift is strongly confirmatory of the views I have here expressed regarding their origin. There is a mixing of species of diverse habitat quite different from the natural grouping, and, . . . having regard to the fact that we nowhere get a pure fauna such as might have lived side by side on the sea floor, it seems a very strong argument against the marine origin of the beds." Mr. Kendall, of course, means their marine origin *in situ*.

Mr. Kendall then goes on to point out what I consider a very interesting and important fact. He refers to the number of extinct species in the Manx drifts. "Of these," he says, "assuming, as I feel justified in doing, that the *Mitra* and the *Woodia* are extinct, we have eight, constituting ten per cent. of the whole fauna. One of these, certainly, *viz.*, *Nassa reticosa*, as I have pointed out, has long been known in the pliocene beds of East Anglia, Cornwall, Belgium, France and Italy, and from the way in which the species diminishes in numerical representation in the successive zones of the Red Crag, and is represented in the newer pliocene deposits of East Anglia only by *remanié* specimens, I feel justified in regarding it as a species which in all probability became extinct, in British seas at least, before the close of the pliocene period. The general aspect of the other species of the Manx drift is southern rather than northern, and these, too, I believe to be pliocene shells.

"One general explanation applies to the occurrence of all these shells, *viz.*, that in common with the rest of the molluscan contents of the drift deposits they are to be regarded not as a contemporary fauna of the glacial period, but as the relics of the shell banks of various geological ages." Of course he attributes their collection and deposition to the ice monster, but with this exception I confess the rest of his argument seems sound, and I will quote it as it stands: "Pliocene shells, which would include probably the extinct and southern species, would be brought into juxtaposition with shells but recently vacated by the occupants of the glacial seas. Shells from the deep water would be mingled with the species which live adhering to the rocks between tides, such as *Purpura lapillus* and *Littorina littorea*; burrowing shells, which would live embedded in mud, would be found along with those whose

natural habitat was a sandy or gravelly bottom. It is consistent with this explanation that the shells are usually in a very much broken condition, only the stronger shells as a rule surviving the rough treatment to which they have been subjected. The univalves are more commonly found in a perfect condition than the bivalves, paired valves of the latter being rarely found, and shell fragments, especially when found in the boulder clays and showing no signs of decortication by chemical action, not unfrequently being polished and ice-scratched." Summing up his opinion, Mr. Kendall says of the evidence of the shells in the Manx drift: "I regard them as relics of shell beds of different geological ages which have been . . . re-deposited either in the unassorted boulder clays or in the washed sands and gravels. A stoneless, shelly clay, containing a purer fauna and better preserved, may be a transported mass of the sea bottom" (*Glacial Geology of the Isle of Man*, pp. 37-43).

Prof. G. A. Cole, speaking of the shell-bearing beds of Ireland, said the "shells are now generally admitted by field observers, including Mr. Maxwell Close himself, to have been removed from their original habitat," and he argues that many of the so-called "recent" species in glacial beds might easily be derived from Astian or even earlier strata (*Quart. Journ. Geol. Soc.*, li., p. 478).

Of the shell beds in Arran, Mr. James Geikie says: "They appear to be to some extent rearranged, so that possibly the shells do not always occupy the positions in which they were originally embedded" (*Great Ice Age*, p. 137). This admission is surely quite enough.

Dr. Fraser has described the occurrence at Oakshaw Hill, Paisley, *beneath a mass of unfossiliferous* till, of a bed of shelly clay with a bed of *Mytilus edulis* on its surface.

Very remarkable evidence of the pre-glacial date of the marine débris in the Scotch drifts is afforded by the beds in Kintyre, which were examined by two special and strong committees of the British Association. They examined the deposits at three places, namely, Tangyburn, Drumore Burn and Cleongart.

At Tangyburn the shelly clay was found to be overlaid by boulder clay thirty feet in thickness. It was similarly overlaid at Drumore Glen.

At Cleongart the section shows (1) boulder clay, seventy-four feet; (2) shelly clay, twenty-seven and a half feet; (3) coarse sand and gravel, eleven feet, and mica schist. The shelly clays in this district would seem to be *in situ.*

Let us now turn to the Continent. In the countries round the Baltic we have a similar problem to solve to that which I have just been discussing in Britain. There also we have a number of drift beds containing or associated with marine shells and other marine débris whose age has been fought over, and the question has continually arisen, and still induces a warm polemic, whether they are pre-glacial or not in the sense used by the glacialists. In order to settle such a point we must decide, as Schröder says, two matters: first, whether they are derivative or not, and, secondly, whether they are both underlaid and overlaid by true boulder till. In regard to both these issues there is the greatest uncertainty, and there has been a very loose method of applying a test and criterion to the problem. Let me first quote an authority of the first class who is a champion of the glacial theory. Wahnschaffe, in his well-known memoir entitled *Die Ursachen der Oberflächengestaltung des Norddeutschen Flachlandes*, says: "There are great difficulties in ascertaining the true horizon of the beds of marine and fresh-water origin as well as beds of peat found by Berendt, Ebert, Jentsch, Klebs, Noetling and Schröder in the diluvium. These difficulties arise from the fact that a great part of the marine shells thus found are derivative, and that among those who have described them there is as yet no agreement in regard to some of them as to whether they are *in situ* or not."

Prof. Geikie quotes the statement of Penck that most of the shells in the North German marine drifts are broken and abraded and that the species is often difficult to determine. Berendt figured a specimen of *Cardium edule* which is distinctly striated. Penck found a similar specimen in the boulder clay of Marienburg in West Prussia, and the geological collection in the University of Breslau he says contains another. The same geologist mentions another notable fact: the shells are sometimes filled with a material different entirely from that of the till in which they lie embedded. Thus a specimen of *Paludina diluviana* in the coarse boulder

clay of Rixdorf, which lies a few miles east of Berlin, was filled with a fine ductile clay, and a *Nassa reticulata* from the boulder clay of Dirshau in West Prussia was filled with fine sea sand. It is evident, indeed, that all these sporadic specimens of molluscs are merely erratics, like the glaciated stones among which they occur. They have been derived from some pre-existent beds (*Prehistoric Europe*, p. 278).

Let us now turn to the lowlands of North Germany. In all this district the diluvium is very poor in shells. In the whole district between the Elbe and the Oder there is only one fresh-water deposit with shells; along the Vistula and through East Prussia only one salt-water fauna, and this belongs not to the present Baltic but to the German Ocean fauna. None of the larger shells, not even the very thick ones, like *Cyprina islandica* or *Venus virginica*, are found whole, while the small ones are described as full of mud or clay.

The marine beds of this district have been divided into two classes, according as they contain the shell *Yoldia arctica*, which Schröder describes "*als wahrscheinlich einer älteren und faunistisch anders charakterisirten Bildungsperiod des Diluvium angehŏrig*" (*op. cit.*, p. 232), and another shell fauna characterised especially by *Cyprina islandica* (smaller variety). This last alone Jentsch considers to be interglacial. "Thus," he says, "the interglacial marine fauna of Prussia is composed of *Cardium edule, C. echinatum, Tapes virginica, Cyprina islandica* (larger variety), *Tellina baltica, Mactra subtruncata, Mytilus edulis, Scrobicularia piperata, Mya* (species), *Corbula gibba, Nassa reticulata, Cerithium* (species), *Littorina littorea* and *Scalaria communis*. Never is *Yoldia* found in these beds, which have yielded many thousand specimens of their more temperate species" (*ibid.*, p. 222).

In regard to the *Yoldia* beds Geikie says: "*Under* what is considered to be *the lower boulder clay* of the low-lying Baltic coastlands of East Prussia, as at Steinort, Reimannsfelde, Lenzen, Succase and Tolkemit (all on the Frische Haff), there occur certain bedded clays which have yielded arctic and boreal shells (*Yoldia arctica, Astarte borealis, Cyprina islandica*), the arctic seal (*Pagophilus grœnlandicus*), a cetacean (*Delphinus* sp.) and remains of a cod" (*Great Ice Age*, p. 441). These beds are declared by Schröder to be *in situ* and

not derivative (*op. cit.*, p. 222). Munthe, who is a great advocate of interglacial beds, and sees them almost everywhere, allows that the so-called *Yoldia* clay is pre-glacial (*Bull. Geol. Inst. Ups.*, iii., p. 28).

This last-named shell belongs to very high arctic species, and is no longer found in latitudes so low as North Germany. Turning to the more temperate fauna, we may divide the south Baltic lands into two marine provinces. First, that to the east of the Oder. There we find marine shells in some of the sands. These shells form a very poor fauna, and, according to Schröder, consist of the following forms only: *Mytilus edulis, Cardium edule, C. echinatum, Cyprina islandica, Astarte borealis, Mactra subtruncata, Mactra solida, Tellina baltica, Scrobicularia piperata, Corbula gibba, Nassa reticulata, Scalaria communis* and *Littorina littorea.*

These, besides some shells now found in the Eastern Baltic, comprise others not now living there, but are like those living in the Western Baltic, as in Kiel harbour.

In a paper published by H. Schröder on two new finds of marine diluvial shells in East Prussia he discusses the question of the existence of real interglacial beds in the districts of East Prussia with considerable skill. In regard to the special sections which he describes, they are near Kiwitten, and represent apparently shells *in situ.* It is plain, as he says, that the shell beds underlie the boulder deposits (*op. cit.*, p. 229). In a section at Vogelsang, near Elbing, where the shells are found with their valves united, and are therefore also possibly *in situ*, and which has been described by Jentsch, there is an overlying clay over the shell bed, but no underlying one. The same is the case with the section at Oelmühlenberg, near Heilsberg, first described by Schumann, Berendt and Klebs, and discussed by Schröder (*vide* pp. 231-239).

Now it is a very remarkable fact that the only marine beds (except the *Yoldia* beds) which Schröder admits to be *in situ* in this district are the three just quoted. While of the rest he says: " Zahlreiche Fundorte von Conchylien, aber fast nur auf secondärer Lagerstätte, finden sich auf den Sectionen Dirschau, Elbing, Heiligenbeil, Labiau und Friedland " (Schröder, *Ueber zwei neue Fundpunkte mariner Diluvial Conchylien in Ost Preussen*, p. 222).

Replying to those who have argued in favour of the marine deposit at Jacobsmühle, near Mewe, being *in situ*, Schröder says: "*Dann auch zwei Niveaus mit marine Fauna dort existieren müsten die den Character der jetzt in der Westlichen Ostsee lebenden an sich trügen*". Of the shell beds found by Jentsch at Jacobsmühle, Grünhof and Kl. Schlanz, which he takes to be remains of an impoverished marine fauna remaining *in situ*, and which are overlaid by boulder clay, he says that he differs from Jentsch in considering these deposits as derivative. In regard to the peat bed found by Ebert at Neuenburg, south of Mewe, and described by Jentsch as interglacial, Schröder says it is also derivative, a conclusion in which Wahnschaffe agrees. I will conclude the evidence of the marine fauna of Eastern Germany by another quotation from the same writer.

Schröder, while saying it is far from his intention to deny the existence of an interglacial period in East and West Prussia, nevertheless is anxious to state that no positive evidence is forthcoming to support it. "Ich glaube nur constatiren zu mussen, dass die marine Fauna Ost und West Preussens bis jetzt keinen stricten Beweis dafür lieferte" (*op. cit.*, p. 237).

Let us now turn to the Western Baltic lands, especially Schleswig and Holstein and their borders. The discussion in regard to the true horizon of the shells found in connection with the drift beds of this area is still in active movement, and Dr. Gottsche has introduced a crooked factor into the problem by following Penck, and invoking a glacial period much older than that represented by the lower boulder clay of the older northern geologists, and which seems based upon very shadowy and unsubstantial arguments, and is not generally accepted. Apart from this, the beds themselves are full of doubt and difficulty. We who have studied them very closely in England know how difficult, nay, impossible, it is to make out any coherent sequence in certain places for the various clays and sands which are so intermixed; pockets or masses of each being so often included in the other. Dr. Gottsche says truly: "Die genauere Altersstellung dieser marinen Gebilde innerhalb des Diluviums zu entscheiden, ist leider in vielen Fällen an der Mangelhaften Beschaffenheit der zu Gebote stehenden Aufschlüsse gescheitert"

(*Die End Moränen und das Marine Diluvium Schleswig-Holsteins Geog. Gesells. in Hamburg*, xiv., p. 4). Dr. Gottsche mentions seventy-two places in Schleswig-Holstein where, when in 1898 he published his two long and learned papers, marine remains had been found in connection with the drift beds, a large proportion of them having been his own discoveries. In regard to thirty of these, which he enumerates in a table (*ibid.*, pp. 11-53), he rejects their evidence as that of secondary deposits, or for other reasons which make their testimony valueless, leaving forty-two localities, where he tells us he had found marine diluvium *in situ*. Any one who looks at the details as he gives them will see how very necessary it is in a great many of these forty-two cases that a careful re-examination should be made in regard to whether the shells in them are derivative or not.

Let us, however, take them as Dr. Gottsche takes them. Of the forty-two he excludes seventeen, which he again enumerates (*op. cit.*, p. 55). These are, he tells us, "*für die Altersfrage ganz aus dem Spiel, weil ihre Lagerungsverhältnisse ungenügend bekannt sind*". This leaves us twenty-five only, out of the seventy-two, which he deems worthy of real discussion, and he is, it must be remembered, a champion of interglacial climates.

Two of these, the beds at Itzehoe and Rögle Klint, he says are "sicher alter, als Unterer Geschiebemergel, *aber fraglich, ob interglacial oder præglacial*". Of twelve others, those at Esbjerg, Hvidding, Nindorf, Farnewinkel, Burg i. D., Warringholz, Cleve, Rensing, Glinde, Mommark, Kekenis and Habernis, he says: "Wahrscheinlich älter, als Unterer Geschiebemergel, *aber fraglich, ob interglacial I. oder præglacial*"; and he adds in a note a delicious sentence, quite a psychological curiosity when referring to the word *wahrscheinlich*: "Nur auf Vorsicht sage ich hier wahrscheinlich; meine subjective Ansicht geht dahin, dass die marinen Thone dieser 12 Lokalitaten sicher (*i.e.*, certainly) alter sind, as U.G. wie ich das ja auch namentlich für den Cyprinenthon oben betont habe". That is to say that fourteen of the remaining beds are *certainly* older than the lower boulder clay.

After deducting all these localities about which Dr. Gottsche expresses his doubts so quaintly, we have eleven localities where he considers it as more likely that the deposits are

interglacial, and these it will be well to examine a little more closely.

The first of them is the deposit at Tarbeck in Holstein, consisting of a bed of shells of *Ostrea*, *Mytilus* and *Buccinum*, first described by Forchhammer in 1835. The sections which have been published of it show it to be covered by boulder deposit, while no such deposit has been found below it. Beyrich, Meyn and Torell *all describe it as preglacial.* Gottsche argues on apparently transcendental grounds that it is interglacial, while Munthe coolly says, in spite of his own section, "Im allgemeinen hält man ihn für interglacial," which sentence he follows up by another, which I must quote: "*Wovon er unterlagert wird, ist allerdings noch nicht bekannt*". This is a fine specimen of the reasoning of the devotees of the new faith. Mr. J. Geikie calls the bed interglacial. At Blankenese the original profile described by Dr. Poulsen is now covered up. In the published profile there is no underlying drift deposit at all, the surface bed alone being labelled *Geschiebe Sand*, while in Gottsche's own boring in 1889 we have the plainest proof that the shell beds with oysters are overlaid by drift beds. No drift bed of any kind was found below them (see Gottsche, *Sonderabdruck aus den Mitt. der Geog. Gesell. in Hamburg*, xiv., pp. 27, 28). With evidence like this before him Gottsche in summing up his results speaks of the beds at Tarbeck and Blankenese as "Sicher jünger als Unterer Geschiebemergel und *sicher interglacial*" !!!

In 1846 Bruhns described a similar deposit with oyster shells at Stöfs. No profile of this deposit seems to be extant. Johnstrup described it as an insertion in the boulder clay. The whole section is destroyed and tumbled about. Herr Gottsche on this evidence says of this section, "*Sehr wahrscheinlich interglacial*" !!!

In regard to the deposit near Sonderburg, in the island of Alsen, described by Dr. Munthe at considerable length in his *Studien*, ii., pp. 53-75, I cannot understand his section, unless it means an inclusion, like so many of the lenticular masses of sand in our drift clays. I may add as a singular fact that, although protected from weathering by a clay bed, Dr. Munthe tells us no shells were found in the sand, and his conclusion that the sand in question contained *Cyprinæ* is based

on the microscopic examination of some bits of epidermis which were treated with reagents. The bed contained a great number of *Diatomaceæ* however (Munthe, *op. cit.*, pp. 55, 56).

With the bed at Sonderburg Dr. Gottsche couples those at Hostrupholz and Fahrenkrug, and says of all three, "Möglicher Weise interglacial II.," and then adds in a note, what is a key to the whole position, "*Dass Hostrupholz und Fahrenkrug sehr wohl älter sein können, ist oben schon angedeutet*," and thus of the long list of seventy-two localities in Schleswig-Holstein where marine beds occur only four remain of which Dr. Gottsche can say, "*Sicher älter als Unterer Geschiebemergel und sicher interglacial*". These four are Dockenhuden, Nienstedten, Hamm and Lauenburg. I do not propose to discuss them here, for they involve problems that will face us later on. In three of these localities the lower clay as tested by borings occurs at the enormous depth of 150 to 180 metres, and it seems as plain to me as possible that the diluvial beds which overlie this clay are not in any way *in situ*, but have been thrown down into a vast hollow caused by some collapse. Here I will only say that the surroundings of the beds entirely exclude them in my view as test objects of the problem. Of one of them, however, the deposit at Lauenburg, Schröder quoting Keilhack says, "The cardium sand of Lauenburg is under the boulder clay and lies directly upon the tertiary beds," which seems to me conclusive.

It is plain, in fact, that the supposed evidence furnished by the remains of marine and terrestrial beds of North Germany to the existence of any interglacial mild climate melts away when it is analysed, and the conclusion seems inevitable that, as in England, these remains are older than the drift period, or, to use the phrase in fashion, are pre-glacial. It would seem, further, that, as in England, they belong to more than one phase of the so-called pre-glacial period. Some of the beds, as with us, are marked by the presence of a specially arctic fauna, of which the molluscs *Yoldia arctica, Cyprina islandica* and *Astarte borealis* are the significant forms. The other beds comprise a marine fauna poor in species but richer than that of the Eastern Baltic, and not unlike that prevailing in the Western Baltic, notably in Kiel harbour and in the Cattegat.

Let us now turn to Scandinavia. The first person who

claimed to find evidences of more than one ice period in Scandinavia was, I believe, De Geer, who wrote a paper in 1884 entitled "*Ueber die zweite Ausbreitung des Skandinavischen Landeises*". Others have since followed in his wake, but Holz has always opposed the view.

First, we may consider the so-called *Yoldia* beds of Sweden. These beds, with their exceedingly arctic fauna, can hardly be of other date than the similar beds in the Frische Haff, which we have already discussed. We have shown that there they are earlier than the distribution of the so-called lower moraine, that is, are pre-glacial. The *Yoldia* clay or marl of Sweden is what Erdmann called glacial clay. It occurs in many parts of Sweden as far north as Dalecarlia and Jemteland and passes into what the same writer calls glacial sand. It is particularly developed in the districts of Bohuslan, West Gothia and Halland, where it is marked by the following very decided arctic shells: *Yoldia arctica, Yoldia pygmæa*, var. *gibbosa, Leda pernula, L. caudata, L. myalis, Nucula tenuis, Mytilus edulis, Pecten islandicus, Astarte arctica, A. sulcata, A. compressa, Cyprinus islandica, Arca varidentata*, var. *major, Saxicava rugosa, S. arctica, Tellina proxima, Lucina flexuosa, Corbula gibba, Anomia ephippium, Natica Grœnlandica, N. clausa, Buccinum grœnlandicum, Fusus despectus, F. Turtoni, Trophon clathratus*, var. *major, Mangelia declive, Balanus Hameri, B. porcatus, B. crenatus, Asterias* sp. The shells abound most near the coast and become fewer as we go inland, and are found from the northern extremity of Bohuslan to the southern one of Halland (see Erdmann, *Formations Quaternaires de la Suède*, p. 82).

That the shells in these beds are not *in situ* over a large part of the district where they occur seems plain from the fact that the remains of land animals and deep-sea mammals have been found with them. Thus at Marieberg, near Uddevalla, there were found in the beds the horns of a red deer, some bones of a young whale (*Hyperoodon*) and some well-preserved remains of a fox.

Again, on the banks of the Risa, near Ofia Bräche, twelve and a half miles east of Uddevalla, about eighteen or twenty feet deep, were found a skeleton of a fish (probably *Gadus morrhua*) and also some bones of a bear. In other places

skeletons and bones of whales and other marine animals have occurred: thus a whole skeleton of a whale, now preserved at Upsala, was found 300 feet above the sea level in the parish of Wanga in Western Gothia. It was described at the beginning of the eighteenth century by Swedenborg. Prof. Lilljeborg identifies it as that of *Hunterius swedenborgii.*

In 1867 a well-preserved skeleton of a seal was found at a depth of twenty-three feet between Wenersborg and Uddevalla. Remains of another whale, identified with the Greenland whale (*Balæna mysticetus*) by Lilljeborg, were found in 1860 at a height of sixty or seventy feet above the sea level, near the town of Warberg in Halland. Other remains of whales were found in the glacial clay of the same province about ten kilometres east of Falkenberg.

A skeleton of another species of whale, namely, *Eschrichtius robustus*, was found at two to four feet from the surface on the island of Gräsö, outside the little town of Oregrund, near Stockholm. *This was found partially in the glacial clay and partially in the clay marked by Mytilus edulis and Tellina baltica* (Erdmann, *op. cit.*, pp. 83, 84).

These facts seem to make it plain that the so-called glacial clays of Sweden are very largely derivative and not *in situ*, which is also proved by the mixture of glacial shells, like the *Yoldias* with those of *Mytilus*, etc., belonging to temperate conditions, thus giving the deposit the same *remanié* character which the Danish geologists have assigned to the *Yoldia* clay of Jutland (see Holst, *Hat es in Schweden mehr eine Eiszeit gegeben*, p. 238, note).

Let us now again cross the Atlantic. I find it very difficult to unravel the marine drift beds of America as described by various writers. The beds containing marine shells occur only in Eastern Canada and the seaboard of New England. There are no shells further west than near Kingston on the St. Lawrence and Arupri on the Ottawa (Dawson, *Post-Pliocene Geology of Canada*, pp. 4, 5), and the clays of the basin of the great lakes to the south-west have as yet afforded no marine fossils (*ibid.*, p. 58). As a large proportion of both sets of beds, both those in the east and west, are stratified, and were therefore deposited by water, this seems to prove that, as in England and on the shores of the Baltic, we have

not to do with a general submergence for a considerable time but with some transitory movement, and that the shells are not *in situ* but *remanié* and derivative, just as they have been found to be in the European beds. The absence of any pleistocene shells in the surface beds of Nova Scotia, as is attested by Sir Wm. Dawson, seems to be a strong argument against the general submergence of the district of the St. Lawrence and its borders. That the beds containing shells which occur in Canada, in New Brunswick and the Northern States of America are stratified shows on the other hand that they were deposited by water. This is further evidence that these shells are *remanié* and have been transported from the sea landwards. This is, again, confirmed by the fact that the western beds answering to these marine clays and sands are barren.

This, again, seems to be proved by various other considerations. Thus in some places the beds not only contain marine shells of an arctic type, but also the leaves and débris of land plants.

These débris especially abound, according to Dawson, in the *Leda* clays at Green's Creek on the Ottawa. The species hitherto described are *Drosera rotundifolia*, *Acer spicatum*, *Potentilla canadensis*, *Gaylussacia resinosa*, *Populus balsamifera*, *Thuja occidentalis*, *Potamogeton perfoliatus*, *Equisetum scirpoides*, *Carices* and *Graminæ* in fragments, *Fontinales* species, *Algæ* (*ibid.*, pp. 99, 100).

"These plants," says Dawson "occur in the marine *Leda* clay, containing its characteristic fossils."

At Rivière du Loup Dawson found in the *Leda* clays the pollen grains of firs and spruces (*ibid.*, p. 63).

Secondly, both the *Leda* clays and *Saxicava* sands are scantily supplied with shells, except at the junction of the clay and sand, where they abound in some places, showing a process of sifting not usual with shells *in situ.*

Thirdly, the shells from different depths, littoral and deep shells, are mixed in these beds in a way so far as I know quite impossible if the beds were *in situ.* In such a case the shells would be zoned. All this makes it probable that the beds with marine shells we are discussing are not marine beds *in situ*, but that their contents are derived and they have themselves been moved. The marine beds have been divided into

three horizons by Dawson and others, namely, true boulder clays, the *Leda* clays and *Saxicava* sands. This division is not unnatural if we adopt it on mere physical considerations. The boulder clay is described as true till, consisting of hard clay filled with stones and thickly packed with boulders and for the most part *destitute of stratification.* The *Leda* clays and *Saxicava* sands on the other hand are mainly stratified.

When we test them by their contents, however, and thus try to find any relative age for them, the test utterly fails. "At Isle Verte, Rivière du Loup, Murray Bay, Quebec and St. Nicholas on the St. Lawrence," says Dawson, "the boulder clay is fossiliferous, containing especially *Leda truncata*, and often having boulders and large stones covered with *Balanus hameri* and *Bryozoa*" (*op. cit.*, p. 7).

In regard to the so-called *Leda* clays and *Saxicava* sands the distinction based on their contents seems arbitrary, as *Saxicavas* and *Ledas* occur in both beds, and Dawson himself says (p. 20) that it is somewhat difficult to refer a large part of the shells to either deposit.

The clays so named often occur separately, and where there is some overlap this is natural in beds which are derivative. The sands and clays when *in situ*, no doubt, had different molluscous contents, due to their feeding grounds being different. They became mixed and confused in places during the transport, but they seem to me to be just as contemporary as adjoining mud and sand deposits on our own shores. The movement of these shells, etc., was, it seems to me, clearly contemporaneous with that of the erratics. Dawson himself says that large travelled boulders often occur in the *Saxicava* sand (*ibid.*, p. 20), and he speaks of large Laurentian boulders as being imbedded in the *Leda* clays (*ibid.*, p. 19).

As a good test case of the actual horizon of the shell beds I will quote the following from Colonel Grant's paper on the pleistocene shells of Anticosti. "The shells occur," he says, "in the blue clay *in situ*, and are capped by a considerable thickness of drift boulders," etc. The shells, of which he gives a list, he says are unusually large. About eight miles from English Bay is a blue clay with very large shells of *Mya*. These occur in a cliff which *is crowned by drift deposits.* When they slope the boulders or rounded pebbles from the top get

mixed up with the clay below. Fragments of shells are here numerous; complete specimens are few (*Canadian Record of Science*, ii., p. 45). He says further that the *Leda* clay in the island contains many specimens of *Saxicava rugosa* (*ibid.*, p. 46), and he adds that the same clay there is *overlaid* by a boulder deposit connected with the *Saxicava* sand. To this it is probable that many of the travelled boulders of Laurentian rocks belong, as they are found in this connection not only along the whole south shore of the St. Lawrence, but even in Prince Edward Island and in Nova Scotia (*ibid.*, pp. 47, 48).

In the Drumlins at Winthrop, in the islands of Boston harbour and on the peninsula of Nantasket, many fragments of marine shells, of which about twenty species have been identified, and all of which are now living in Massachusetts Bay, occur in the unstratified glacial drift or till in the same manner as its boulders and rock fragments, and Upham considers them as derivative. *Venus mercuraria*, the round clam or quoliog, is the most abundant species (*Amer. Geol.*, iii., p. 399).

This will suffice in regard to North America. In regard to South America the problem has still to be examined on the spot. The only fact I have found is a recent statement by Nordenskiold, who, in describing the recent beds of the Magellan territories, tells us that in the boulder formation he found no remains of life except occasional shells of a *Turritella*, "common in the underlying tertiary deposits. These are presumably not original here" (*ibid.*, p. 304).

Let us now try and realise where we are. I hold, then, the shells in the various stratified surface beds of Eastern America to be not glacial (in the sense in which that word is generally used), but derivative and pre-glacial, just as the corresponding beds in Europe have been shown to be, and that they represent the fauna of the pre-glacial sea just as the plant remains in the same beds represent the flora of the adjoining pre-glacial land, and when the transport took place by which the clays and sands and shells were moved the débris of the existing land flora were incorporated with these marine deposits. As we proceed inland the quantity of the marine débris gradually gets less, and, as we have seen, it definitely ceases at a certain point, which we have already described. We have already seen that the remains of the land fauna or flora afford no sub-

stantial foothold for any theories of interglacial periods. The same is the case with the mollusca found in the drift beds. Wherever we can test the case it seems to follow that they are derivative and belong to a marine fauna contemporary with the mammoth age and the age of paleolithic man on the land.

Nowhere can we find biological evidence to sustain interglacial mild periods. Wherever tested and analysed this evidence has broken down, and if there be some cases where a doubt exists they are so few and sporadic that I prefer to treat them as involving faults of observation rather than as being exceptions to the rule. This is not all, however: a more important conclusion remains behind. It seems to me that not only does biological evidence of interglacial mild periods fail us, but that there is no evidence of a glacial period at all if we limit ourselves to the biological case alone. If all the remains of life in the drifts are derivative they can afford no testimony at all about a glacial *age*. They can tell us of the kind of conditions there were before the drift was distributed, just as the later fauna can tell us of the corresponding conditions after it had been laid down, but in regard to any *periods* marked by glacial conditions their witness and testimony is a blank. This is surely a very important factor in the problem. We know at present no seas so cold that they do not contain some life and few lands, and if the drift beds testified to a long continued period and not to a mere transient phase we ought to find some relics somewhere of the fauna and flora which characterised it. I know of none. The only answer available, I take it, is that from which some ardent glacialists will perhaps not shrink, namely, that all life, both terrestrial and marine, was absolutely exterminated at that period north of lat. 50, and that every sea was then frozen tight to its bed in the temperate zone and every acre of land was smothered with snow and ice, like a portentous pall of death, so that life became impossible. To those who hold this view the biological testimony and the conclusions I have set out speak no parable whatever. To them it is another proof that their armour is invulnerable. To anyone else who cares for the discussion it must cause doubts and difficulties galore, and to some it must look more than ever that the glacial theory is a fantastic dream.

CHAPTER XII.

THE ALLEGED SOUTHERN FRONTIER OF THE DRIFT BEDS AND THE MISTAKEN INFERENCES BASED UPON IT.

"The whole country thereabouts is a solid body of chalk, covered with most delicate turf. As this chalky matter hardened at creation it spewed out the most solid body of the stones, of greater specific gravity than itself, and, assisted by the centrifugal force, owing to the rotation of the globe upon its axis, threw them upon its surface where they now lie."—Stukeley on Sarsens (*Abury Described*, p. 16).

In the previous chapter I have examined the alleged biological evidence in regard to an ice age, and have endeavoured to show that there is no such evidence forthcoming anywhere and that the remains of plants and animals which have been adduced from the drift beds as evidence of glacial periods, or of interglacial periods, are derivative and really belong to the horizon below the drift, and therefore are older than that deposit.

It would be natural and in order if I were now to examine the inorganic constituents of the drift, and to subject them to a similar analysis; but before doing so I think it necessary to face another problem, which seems to me to have been entirely misstated and misapprehended by the champions of the glacial nightmare, namely, the question of the southern boundary of the drift. In nearly every recent manual of geology we have had some wonderful maps published, marking in aggressive colours the area where the glacial phenomena are supposed to have occurred, and this area is limited on the south and east by sharply defined lines of frontier, the supposed furthest limit of the ice monster's footsteps. This frontier line seems to me to be entirely misleading and illusory.

Let us first look at Great Britain. The line in England is drawn along the valley of the Thames, and continued to

the estuary of the Severn. Along this line we nowhere find anything in the shape of a terminal moraine or any other similar feature to show the limit of an ice-foot as there is in the case of a modern glacier, and the boundary, in so far as the surface contour of the drift beds is concerned, is a purely arbitrary one.

The only justification for laying down any such line is not the existence of surface features justifying it, but the supposed difference in contents and texture of the drift beds themselves, and notably the supposed absence of erratics to the south of the line in question.

It seems to me that in both respects the orthodox view is quite misleading. The number of crystalline boulders in the drift of England diminishes at a considerable rate as we move southwards from the great mountains of the north, while the proportion of stones belonging to the later and stratified beds increases in a similar fashion. This, by the way, is in great contrast to true glacier deposits where the stones are carried by the glacier, not part of the way but right to the ice-foot. What we find in England is that as we leave the north, with its crystalline rocks or such centres as Mount Sorrel, in Northamptonshire, the granites, gneisses, greenstones and other primitive rocks become fewer and fewer, and the blocks and débris of the mountain limestone and carboniferous beds largely take their place and give a peculiar feature to the local drift. These again presently give way to the permian and triassic débris. Presently we get into an area where the oolite and the lias remains prevail, and, lastly, we get into one where the chalk prevails. This last change is a very conspicuous one, since it means that the surface beds become very markedly white or grey, and in consequence they have been separated from the other drift beds in a most inconsequent fashion, and fill a large space in glacial literature under the name of "the great chalky clay" or "the chalky boulder clay". It would be just as reasonable to speak of the liassic clay, or the oolitic clay, or the triassic drift, or the carboniferous drift. All of them would be excellent names to distinguish local varieties of the same great deposit, but would be ridiculous if they were meant to convey that each of them marked a separate horizon as the

chalky clay is supposed to do. This, by the way, as we shall see presently, applies not only to the stones in the clay, but to the clay itself, which changes in texture as well as contents with the change in the beds below it.

When we get to the Thames valley we get to an area where the chalky clay no doubt dies out, as the liassic clay and the oolitic clay die out further north. The reason of this seems to me as plain as can be, namely, that we get into an area where the chalk was once covered with tertiary beds. These tertiary beds are for the most part not argillaceous, as the great secondary strata bordering the chalk often are, but sandy and gravelly; and we quite naturally pass, therefore, from chalky clays with chalk boulders galore, into beds largely consisting of gravel and shingle and sand, with great blocks and boulders, not of chalk, but of greywether sandstone and pudding-stone and angular flints, which, as we find them, are as much boulders as the great lumps of chalk and oolite which we find in East Anglia and the Fen districts. In regard to the primitive erratics, however, it is quite a mistake to suppose the foreign stones do not occur south of the line above mentioned. They become scarcer, it is true, but they have become scarce before we reach that line; and the fact is that one of the main points in my own general argument is that whatever distributed the drift it must have been a force which laid down a large part of its earlier burden in the early stages of its march and gradually left fewer and fewer specimens of its original load as it progressed.

I now propose to devote a little space to the boulders which have been found to the south of the critical line already mentioned, and we will first consider the primitive boulders. These prevail for the most part in the western counties of Devon and Cornwall and along the south coast.

Sir H. De La Bêche, in his report on the geology of Devon and Cornwall, says: "On the opposite side (*i.e.*, the southern side) of the Bristol Channel the drift from the north is readily seen, rounded portions of marked rocks, well known to occur on the north, being found not alone in valleys, but on hills and on their flanks, where no rivers, such as would flow from that land from the present inequalities of its surface, could

produce the deposits of gravel and boulders there seen. A great mass of detritus seems to have been swept into the Bristol Channel from the northwards, and this we could scarcely suppose would happen without a great body of water passing onwards to the southward carrying before it, when it struck the opposite shores of the Bristol Channel, a large proportion of the disintegrated or decomposed surfaces of rock" (*Geology of Devon and Cornwall*, p. 400).

At the base of the sandstone cliffs at Saunton Down and Croyde, in North Devon, there occur a number of large boulders of various kinds of rock. Some of these, it would seem, says Prof. Hughes, do not exactly resemble any rock masses in the drainage areas from which they could have been transported to where they are now found by any kind of rain action. Pengelly, who especially referred to one great boulder of red granite, considered that it was beyond the power of the tide to have moved it, and invoked the aid of floating ice.

Of the boulders which occur along these cliffs some were probably derived from Devonshire, but three of them at least have apparently come from a much greater distance. One of these is a mass of yellowish-white gneissose granite, the part exposed measuring 8 × 6 × 6 feet, and is situated a quarter of a mile west of Middleborough. The second boulder occurs about half way along the shore, south of Saunton Down. It is of grey prophyry, like that at Arenig. It is four feet across from corner to corner. The third boulder is like the Ross of Mull granite. Mr. Whitley describes it as egg-shaped and containing rose-coloured crystals of felspar. It has been estimated by Mr. Townshend Hall to weigh from ten to twelve tons. Mr. Hall thinks it very doubtful whether there is any vein of a similar colour and texture on Lundy Island capable of producing a block of such magnitude. The nearest point of Dartmoor is thirty miles off, and red granite only occurs sparingly and much further off. The Rev. Dr. Williams (*Trans. Geol. Soc.*, v., p. 287) was of opinion that it could not have come from Dartmoor or Cornwall or Lundy, but that it resembles the granite of the Grampians.

A propos of these stones, which seem to an unprejudiced person as much ice-borne stones as those of more northern

latitudes, Mr. T. Hall said he was of opinion that "in various parts, both of Dartmoor and Exmoor, there are collections of stones and débris similar in every respect to those composing the moraines of modern glaciers and valleys, which have evidently been shaped by glacial agency".

Among the other evidences quoted may be mentioned that Mr. Doe (*Report of the Committee on Erratic Blocks to the British Association*, 1876, p. 110) records some boulders of felsite occurring at a height of some 500 feet above the sea near Great Torrington, but these are said to be like the elvan.

Mr. Hall states that at Waddeton Court, near Dartmouth, a group of new red sandstone boulders are found reposing on the slate at elevations varying from eighteen to near 200 feet above the level of the sea. At Haberton, near Totnes, also on a slate sub-soil, boulders of a fine-grained trap occur at a height of about 100 feet, and are especially noticeable as being in some cases marked with parallel grooves or scratches. Another group, also composed of trap, is situated at Druid, near Ashburton, and boulders of various sizes have been recorded as occurring in the parish of Teignton, near Teignmouth, some 300 feet above the sea. Another instance of transported boulders occurs in the parish of Fremington, near Barnstaple, where boulders of trap are frequently found on or near the surface of a thick bed of brown clay much used for pottery.

"Dr. Slade King," says Prof. Hughes, "also informs me that a water-worn boulder of coarse grey granite, weighing, say, two cwts., was found in a clay deposit in draining a meadow on Becklescombe farm, near Ilfracombe. It was round and smooth, and had no grooves or striæ on it" (*Quart. Journ. Geol. Soc.*, 1887, pp. 657-670).

On the highest parts of Blackdown, and on the insulated summits round the vale of Charmouth, pebbles of fat quartz are found abundantly, which must have been drifted from some distant primitive or transition country and carried to their actual place. "These cases are precisely similar with those of the blocks of granite on the Jura and the plains of North Germany and Russia, and with the quartzose pebbles on the tops of the hills round Oxford and Henley" (*Geol. Trans.*, 2nd series, i., p. 102).

Mr. Henwood mentioned the occurrence of boulders at

Morral Place, Penzance, at eighty feet above sea level. At Lamorna Cove Mr. Carne noticed boulders of immense size in the roof of a cavern ten feet above the highest tide. Between Lamorna Cove and Mousehole he observed three to eight feet of boulders under thirty feet of angular fragments in clay to the north-west of Carndu.

Near Carn Bargis he noticed a mixture of boulders and angular fragments about twenty feet above high-water level. The roof of Gamper Hole is composed of granite boulders, under twenty feet of granite fragments in clay, and is forty feet above high-tide level.

At Pedn-men-du Mr. Carne observed a few boulders at thirty feet above the sea. The same observer records the occurrence of a bed of boulders fifteen feet thick, in a disintegrated granite and clay matrix, at fifty feet from the surface, and about 500 feet above the sea, in sinking a shaft at Huel Carn tin mine.

Mr. Salmon mentioned the discovery of granite boulders at seventy-four fathoms from the surface in West Rosewarne mine, Gwinear, which he thought had been introduced by fissures from the surface.

Mr. Whitley gives a sketch section from Lennor Castle to the coast on the north of it, showing boulders of granite on the surface of a bed of decomposed granitic loam, thin on the slope, but as much as ten feet thick on level ground, and resting alike on the granite and kellas. Though the above may be due to early pleistocene denudation, it is more probably ascribable to the period during which "the head" was accumulated. Diallage boulders are similarly separated by soil from their parent rock in the Lizard district (Ussher, *Post-Tertiary Geology of Cornwall*, pp. 15, 16).

West of the granite tors of Dartmoor the quartz pebbles are in structure like the large boulders of quartz known as "the Whitakers," which abound near Upland, in the parish of Tamerton Foliot, about five miles north of Plymouth. One of these latter great quartz boulders is egg-shaped, about ten feet by five, and lies half buried in a mass of smaller boulders and clay. Mr. Pengelly, F.R.S., in his description of the Whitakers, says: "That the blocks have travelled a considerable distance cannot be doubted, and some of the

blocks, instead of lying at once on the Shillet, were lodged in a heterogeneous accumulation of clay and stones, including Whitakers, from the size of an ordinary apple to that of a common cocoanut" (*Trans. of the Devon. Assoc.*, vol. xii., p. 311).

According to Whitley, a boulder clay underlies the drift beds at the mouth of the Taw. "I have particularly noticed," he says, "at the section under Heanton Court, that the egg-shaped boulders with which it is loaded are pitched upright in the clay, with their small ends downwards, as if they had descended like a parachute through deep water into the soft, recently deposited clay. On further examining the coast line, I found at low water, pitched in the clay, extending far seawards, of the northern pebble ridge, long, slender boulders of blue carbonaceous grit, which may be seen standing upright through the thin superficial covering of shell sand, like the naked ribs of a wrecked vessel which is imbedded near" (Whitley, *Flint Flakes*, etc., pp. 10-12).

Belt, in a paper on the drift of Devon and Cornwall, divides the drift beds there into two sections. First, upland deposits, reaching up to 1,200 feet above the sea, consisting of gravels, clays and transported boulders; secondly, often fragmentary patches and lowland deposits, gravels, clays and boulders, spread out in more persistent and regular beds at lower levels. In regard to the former he says: "The whole of the surface of Dartmoor is sprinkled over with large blocks of granite, some of which are of enormous size. . . . If the blocks of granite had been left by the decomposition of the softer bed rock they would have rested on it, not on gravels and sands. Neither can they have slipped down from the 'tors' that crown many of the hills; for they are not confined to the slopes, but are spread out over the level ground as well; neither are they concentrated at the bottom of slopes, as they would have been by such action. Here two or three or even a dozen may be crowded together, but generally they are separated from each other."

Near Okehampton the bed rock is composed of hardened shales and sandstones. The sides of the hills are covered with a stony clay, sometimes as much as thirty feet thick, containing many large angular blocks of stone, especially

near to and on the surface—among these, blocks of granite that must have been brought at least two miles are not infrequent. On the eastern side of Dartmoor Mr. J. W. Ormerod has noticed beds of gravel reaching up to 900 feet above the sea, and has given instances of many transported blocks of granite and carboniferous rocks. Thus to the east of Cranbrook Castle (1,110 feet above the sea), and near Wooston Castle, large transported granitic blocks overlie the carboniferous beds. On the Newton and Moreton Hampstead Railway fragments of granite are spread over the carboniferous strata, and gravel formed of elvanic and carboniferous rocks occurs at Riddy Hill, its nearest point of derivation being on the opposite side of the valley. Mr. George Man has described the occurrence at Petroclistow, near the centre of the county, of an isolated bed of gravel, composed almost entirely of the detritus of Dartmoor granite, twelve miles distant from the nearest granitic mass, and considers that it can only be accounted for by an amount of submergence covering the whole of the ridges. Many miles to the east, on the Haldon Hills, gravels are found containing pebbles of granite and other crystalline rocks mingled with chalk flints and chert. At Little Haldon Mr. Mackintosh found the gravels ten feet thick, extending down the sides of the hill, proving that it had its present contour before they were deposited. The top of Little Haldon is about 800 feet above the sea, and the gravels extend over its summit.

Gravels with foreign pebbles are found on Straightway Hill, and on the summits of the hills surrounding the vale of Charmouth. In regard to the drift gravels that cover the Blackdown Hills, first described, I believe, by Dr. Buckland, Mr. H. Woodward says: "The bed rocks belong to the greensand formation, and on the tops of the hills, at heights of from 600 to 700 feet above the sea, occur patches of gravel with large, well-worn boulders of chert, large pebbles of quartz rolled-flints, and a few boulders of quartzite. The occurrence of old pebbles in a clayey drift on the tops of the greensand heights is the more important, because on the palæozoic rocks of Devonshire it is often very difficult to decide between a drift gravel and the disintegrated conglomerates of the new red series."

In Cornwall, upland gravels occur on Crousa Downs, near St. Keserne, and blocks of syenite are spread over the surface of them. On the isolated hill of St. Agnes, on the northern coast, Mr. Benedict Kitto has described gravels containing angular and water-worn stones covered by clay, and that again by rubble and rounded pebbles.

Turning from the upland to the lowland deposits, which are often sudden and tumultuous, Mr. Belt says: "On the hills large stones are found scattered over the surface; in the lowland deposits they occur at the bottom of the gravels. In Cornwall the stream tin is found at the bottom of the gravel along with rocks of quartz of great size." In regard to other areas in the south of England, Belt says: "Upland deposits have been described on the Black Downs, to the south-west of Dorchester, at Preston and Osmington, and in the Admiralty quarries on the top of the Isle of Portland. The latter are remarkable as they overlie remains of *Elephas primigenius* and *E. Antiquus*. . . . In every county of the area to which this paper is confined (*i.e.*, the counties south of the latitude of the Thames) there are upland gravels with transported stones" (*Quart. Journ. Geol. Soc.*, 1875, pp. 80-90). Prestwich, speaking of Cheselton in the Isle of Portland, mentions the great masses of débris there, and the large blocks of Portland flint which come from beds from 350 to 450 feet above the sea in that part of the island, and he remarks on the great force necessary to move masses of such size (*Quart. Journ. Geol. Soc.*, xxxi., p. 50). The phenomena here enumerated have been directly or indirectly traced to the operation of glacial forces by D. Mackintosh, J. F. Campbell, Belt and others, and they seem to me to be simply the prolongation of the phenomena occurring further north, and nothing else.

Let us now turn to the crystalline boulders found on the south coast in Sussex and Hampshire, which have been such a puzzle to many inquirers, and which still await a rational explanation.

On these boulders I know no one who has written more fully and wisely than Mr. Godwin-Austen. Speaking of the yellow drift clays occurring on the coast of Sussex, he says of the contents of these beds: "Independently of materials

from the chalk formation, there are also occasionally fragments from oolitic strata and of fossiliferous chert sandstones from the upper greensand; but the great peculiarity of this part of the series consists in the presence of rocks which, from their *age, composition, points of origin, size and condition,* render its mode of accumulation a problem of no slight geological interest. The rocks in question consist of grey porphyritic granite (these are the most abundant), compact red granite, syenite, hornblendic greenstone, mica schist, green fissile slates and fibrous chloritic semi-crystalline rocks, masses of quartz from veins, siliceous sandstones such as those which occur in the palæozoic strata (Lower Silurian) of Normandy, coarse siliceous conglomeratic masses from the same series, micaceous sandstones with *Orthides* (Devonian), and black micaceous shaly sandstones, perhaps from some coal measure series. With these are blocks of compact limestone, but whether mountain limestone or from the older middle palæozoic series of Devon and the Cotentin is as yet uncertain." Mr. Austen tells us that some of these rocks also occur in the beds above the yellow clay, which has been much abraded and has supplied materials for them.

Dr. Mantell pointed out that the pebble or shingle bed near Kempe Town contained fragments of granite, porphyry, greenstone, quartz rock and greywacke slate, etc., all now foreign to the district (*Proc. Geol. Soc.*, 1851, p. 364).

Mr. Dixon noticed similar foreign rocks in Bracklesham Bay (*Fossils of Sussex*, p. 14), and Sir R. Murchison met with "a few pebbles of granite" in the lower marine beds between Hove and Brighton (*Quart. Journ. Geol. Soc.*, vii., p. 396).

The form of the rocks occurring in the yellowish clay gravel deposit is either that of perfectly formed elliptical shingle or that of large subangular blocks; the rocks of the granitic series are much the most rounded, some presenting beautifully smooth and polished surfaces; the greenstones, though their angles are worn, retain much of the forms resulting from the jointed structure of those masses. On the other hand, many of the blocks are sharp and angular, as is particularly the case with respect to the quartz and slate rocks. In size these older materials range from coarse shingle up to masses of twenty tons weight; the granitic blocks, which are by far the most

numerous, are also of larger dimensions than the others. Fragments of greenstone are to be met with from three to four feet in length. A rounded block of porphyritic granite has been recently exposed near Pagham by coast-line denudation, at a spot which only a very short time since formed part of a cultivated field; this block measures twenty-seven feet five inches in circumference. They are found wherever the yellow clay gravel occurs; they have a wide range beneath the whole of the level extending towards Selsea, locally known as the Man Wood (? maen wood or stone wood—H.H.H.); they seem most abundant and bulky between Bracklesham Bay and Worthing; their apparent increase about Selsea may be due to the advance which the coast line there makes.

Rounded blocks of large size also occur as far inland as north of the South Coast Railway, and certainly beyond the limits of the yellow clay gravel deposit; but they are covered by other beds. Where such boulders occur in the upper beds, they are outliers, and mark the former extent of the yellow clay gravel beds; and they may also serve as a measure of the moving power of the water, which, when it denuded that area, was insufficient to displace the larger masses.

The smaller water-worn specimens of old or crystalline rocks are of various sizes, such as may be met with on the beaches of the French and English coasts of the western extremity of the Channel, and coast-line materials may, we know, be conveyed to great distances. The formation of perfectly rounded shingle, however, as compared with masses of subangular gravel, implies lengthened attrition in places, such as bays, where the materials remain long impounded. *If we assume that the rounded granitic and other shingle now found in the beds of the Sussex levels may have followed the coast line from Cornwall and Devon eastwards, it should happen that all these materials should be water-worn, that similar pebbles should occur along the intermediate space, and that they should increase in bulk and in number westward; but such is not found to be the case.* Again, Godwin-Austen says, under the supposition that these materials may have been derived from our own western districts, it is necessary that all the different rocks above enumerated should occur in that quarter, and he goes on to

argue rather ingeniously how they may have come from Normandy and Brittany (*Quart. Journ. Geol. Soc.*, xiii., p. 40, etc.).

In his paper on the raised beaches of the south of England, Prestwich seems to imply that foreign rocks do not occur in the raised beaches east of Brighton. West of it, in the raised beach, he found pebbles and worn blocks of foreign rocks, which consisted, *inter alia,* of light grey and red granites, light red syenite, red porphyry (decomposed), dark grey felstone, greenstone (decomposed), mica schist, white, grey and red quartz, olive-green slaty rock, white and grey quartzite pebbles, dark and light green sandstones, red and grey calcareous sandstones, compact brown sandstones, fissile micaceous sandstones, light-coloured limestones, brown shales an oolitic rock, an earthy yellow lime rock, chloritic chalk, dark yellow chert, black chert, ragstone (lower greensand) and jasperoid flint (*op. cit.*, p. 269). Mr. W. J. Abbot, F.J.S., claimed to have found that some of the quartz specimens were ice-scratched.

"Over the plain between Worthing and Pagham, in addition to the pebbles of foreign rocks commonly found as far as Brighton, large boulders of similar rocks are met with in considerable numbers. Near Barnham farm there was a few years ago a block of fine-grained red granite measuring two and a half feet by one and a half. Occasional blocks occur in the direction of Chichester, but it is at and around Pagham harbour that the foreign boulders are found in greatest number and of largest size." On Hayling Island large boulders are common on the coast, and many are to be seen in the farmsteads and at the cross-roads. In 1875 Prestwich found here specimens of fine-grained granite, red granite, syenite, porphyry, diorite, white quartz rock, light-coloured micaceous sandstones, light red sandstones, hard chalk drilled by annelids, freshwater limestone (upper tertiary) and fossil wood (Portland beds). Most of the blocks were angular or subangular; some few were smooth and looked ice-worn; but he adds he saw no definite markings. They are often met with in trenching and draining at a depth of from one to four feet. They may vary in weight from a half to six tons. The two largest blocks he saw were of sandstone, and measured respect-

ively 6 feet 10 inches × 5 feet × 2 feet 4 inches and 5 feet × 5 feet 2 inches × 2 feet 7 inches (this last was very angular). On one mile of shore he counted as many as thirty blocks of various sizes and materials. They must originally have been even more numerous, for of late years a large number have been gathered together in the grounds of Westfield House, while many have been broken up. He adds that he had been much interested in a specimen of fossil wood like that of Portland, twenty-six inches long by fourteen in width, which was shown him at the vicarage; it seemed to him, he adds, at the time almost conclusive evidence of the direction from which the boulders came. Smaller pieces had also been found. The Rev. O. Fisher mentions a similar discovery at Selsey (see *Geol. Mag.*, 1871, p. 524; Prestwich, *op. cit.*, p. 273). Fisher says: "That erratics from the Portland beds have found their way into Sussex I believe to be a fact, for I saw in 1866 a block of indubitable Portland fossil wood at Selsey, in the garden of Mrs. Pullinger. I was informed it had been found on the beach at that part of the shore called 'The Park'. The block was eighteen inches long and ten inches in diameter" (*op. cit.*, pp. 524, 525). He quotes this as a proof that the boulders came from the west. A few feet beneath the surface and under the drift and boulders marine shells (*Ostræa*, *Cardium* and *Littorina*) are said to have occurred (Prestwich, *ibid.*, p. 273).

The furthest point westward, according to Prestwich, that the boulders have been traced is Portsea, though pebbles of the old and crystalline rocks are to be found beyond.

In discussing the origin of the foreign boulders in the raised beaches, Prestwich argues that the straits of Dover were open and not closed when these boulders were deposited, and he connects the boulders of Sussex with the boulders mentioned by Captain Martin as existing off Ramsgate, and assigns both to an ice-bearing current from the North Sea. In regard to the pieces of fossil wood already mentioned which have been derived from Portland, he says he has seen similar wood on the Boulonnais, although not on the coast, and he quotes from Dr. Fitton's description of that district as follows:—

"The line of the cliff from Equihen, on the south of Boulogne, to Cape Grisnez, on the north of that place, is

capped at intervals with a thin crust of the Purbeck strata, resting upon those of Portland, and consisting of slaty beds of limestone which contain freshwater shells, and include a bed of tough, dark-coloured clay in which are numerous fragments of *silicified coniferous trunks not distinguishable from those of the Isle of Portland*" (*Trans. Geol. Soc.* (1836), 2nd series, iv., p. 326). Prestwich argues that the fossil wood may consequently have come from the French coast or from the Varne and Ridge shoals (*ibid.*, p. 297).

Our next authority is Mr. C. Reid, whose paper on the pleistocene deposits of the Sussex coast contains many interesting and important facts.

Reid says, very properly, that it is always necessary to discriminate between true erratics and ballast or other stones derived from wrecks, and he accordingly limits his list to boulders actually found fixed in the clay. He was also the first to describe in detail the boulders of softer rocks as well as the more primitive ones. Of those at Medberry he gives the following list:—

Bembridge limestone from Bembridge Lodge, Isle of Wight.

Bognor rock from Bognor Lodge, greywethers, some probably not carried far.

Upper chalk flints, upper greensand, probably from the Isle of Wight.

Palæozoic, consisting of sand, pale green and reddish sandstone, greenstone and muscovite biotite granite.

From other deposits he mentions Bognor rock, greywether sandstone (sometimes large blocks), greensand chert (small pebbles), hard purple grit, felsite, felspar porphyry, diorite, greenstone granite; coarse-grained gneissose biotite in the Selsey mud deposits, coarse-grained biotite, fine-grained biotite, containing large porphyritic crystals of white orthoclase, hornblendic biotite, or possibly quartz mica diorite.

Reid says the first thing that strikes one is the preponderance of erratics from known localities not more than twenty miles from Selsea, and he says that previous writers do not allude to the occurrence of characteristic Isle of Wight rocks; and he continues: "It now appears that the granitoid rocks are in a small minority, and that the bulk of the

material comes from the Isle of Wight and the Sussex coast, and that the rarity of these when exposed is due to their having weathered and decayed". Among the various boulders described, one, probably weighing over two tons, is remarkable as showing, according to Mr. Reid, signs of glaciation. It is striated on one face and smooth on the other, and consists of Bognor rock full of *Pectunculus brevirostris* and *Voluta denudata*, and is irregularly scored and striated.

He goes on to describe a surface marked by pits or basins cut in the eocene beds on the foreshore. These detached pits were from two to six feet across. Four out of five of these pits contained only gravel and a few valves of *Balanus* and rare fragments of marine mollusca. The others each contained an erratic block whose upper surface was generally flush with the general level. Some of the softer blocks were broken and splintered, and their angular fragments were slightly separated by gravel or fossiliferous eocene clay. These pits, he argues, were made by the erratics fixed in ground ice. He expressly says that no furrows ploughed in the clay were noticed. Besides these boulders at Medmerry farm, others were seen at West Wettering also embedded in eocene clay.

Whatever the explanation of the origin and transport of these stones, it seems clear to me they prove that the Thames is not the southern boundary of great erratics of primitive rock, and that if an ice-sheet is necessary to explain the similar boulders further north, it is arbitrary to exclude these southern stones from its operation. I shall have more to say on this point later on. My present purpose is merely to show that the statements about the southern frontier of the erratics are mistaken.

Before we pass to another class of boulders, namely, the Sarsen stones, I would mention an individual discovery which has a special interest of its own.

Mr. Elsden says: "Not a single instance has been noticed of the occurrence of foreign boulders upon the surface of the Wealden valley itself, but I procured a moderately large granite boulder from the summit of the chalk escarpment, and exactly upon the 600 feet contour of Kilhurst Hill. The occurrence of the boulder," he says, "is so remarkable

that it merits a more detailed description. The mass weighed between five and six pounds, and had an irregular shape, with a rough surface destitute of any signs of scratching or polishing by ice action. When broken it exhibited the structure of a moderately coarse-grained granite, with an abundance of pink and white felspar and small grains of quartz. . . . The section bears a strong resemblance to a specimen of Peterhead granite in my cabinet." He concludes, very judicially, "that the finding of a granite boulder on the summit of the chalk escarpment may, if confirmed by other similar discoveries, considerably modify existing ideas respecting the physical condition of the area during the glacial period" (*Quart. Journ. Geol. Soc.*, 1887, pp. 649 and 656).

Those who have limited the distribution of the erratics to the country north of the Thames have overlooked the *Sarsen stones*, or greywethers, which seem to me to be as much boulders as any other large travelled blocks, and which have been found in the same beds as the crystalline boulders at Portsea, Hayling Island, etc. (see James, *Edin. Geol. Soc.*, ii., p. 156). The importance of the Sarsens in regard to the argument I am at present urging induces me to devote some space to these stones, on which my old friend Prof. Rupert Jones, F.R.S., has written an exhaustive monograph, from which I shall freely quote. The first notice of them which he refers to is from Richard Symond's *Diary of the Marches kept by the Royal Army*, published by the Camden Society. There we read: "Tuesday (Nov. 12th, 1644), . . . to Marlingsborough, where the King lay, . . . the troops to Fyfield, two myles distant, a place so full of a grey pibble stone of great bigness as is not usually seene; they breake them, and build their houses of them and walls, laying mosse betweene, the inhabitants calling them Saracens' stones; and in this parish, a myle and a halfe in length, they lye so thick as you may goe upon them all the way. They call that place the Grey-weathers, because a far off they looke like a flock of sheepe" (*ibid.*, p. 151). In Aubrey's *History of Wilts*, written about 1656-84, he speaks of "the greywethers which lye scattered all over the downes about Marlborough, and incumber the ground for at least seven miles in diameter," and says, further, "these downes look as if they were sowen with

great stones very thick, and in a dusky evening they looke like a flock of sheep, from whence it takes its name. One might fancy it to have been the scene where the giants fought with huge stones against the gods." Elsewhere he says: "I have mett of this kind of stone sometimes as far as from Christian Malford, in Wilts, to Abindon, and on the downes about Royston, etc., as far as Huntingdon, are here and there those Sarsden stones". In the Marlborough Corporation book for 1673 we have an entry, "Paid for two loads of Sarazen stones, 8d."

There has been some discussion as to the name "Sarsen" stones. Some will have it the word is "Saracens'" stones, *i.e.*, heathen or pagan stones, and Long urges that on the continent "Sarrasin" is commonly given to any roads and buildings of Celtic or Roman construction and not Christian, while Bristow, who supports the contention, says that in some parts of England "Saracen" simply means foreigner, and in Cornwall large heaps of refuse from the mines are known as "attle Saracen," or rubbish heaps, left by the Saracens.

Others have urged that the word is really "Sasen" or "Sassen," the plural of *ses*, Anglo-Saxon for a stone, and connected as a root with the Latin *saxa*. Mr. Adams again urges that the name is derived from the Anglo-Saxon *sar* (grievous, troublesome; Scotch *sair*) and *stan*, a stone, from their causing much trouble to those who first cleared the ground. Others, again, have suggested that the name is a place name. There is a village Sarsden in Oxfordshire, three and three-fourths miles from Chipping Norton, while Sarson, or Sarsden, is a tithing of Andover in Hampshire.

To continue, however. "The first scientific man known to me who referred to these stones was William Smith, the father of English geology, who deemed them the harder portions of the tertiary strata once covering the chalk, which were left behind when the softer portions were washed away" (MS. note of Mr. Long, quoted by Rev. A. C. Smith in his *Memoir*, p. 27). In 1819 Greenough wrote of them: "The greywether stones so plentifully scattered over the southern counties of England are evidently derived from the destruction of a rock which once lay over the chalk" (*Critical Examiner*, etc., p. 112).

Dr. Buckland writes thus about them: "In the interior of Dorset and in the counties of Wilts and Berks the surface of the chalk . . . is occasionally strewed over with enormous blocks of siliceous sandstone, the wreck of strata, whose softer materials have been entirely washed away. These blocks have been long noticed by the name of Sarsden stones and greywethers on the downs of Wilts and Berks, and are particularly abundant near Marlborough, at Kennet on the west, and in Savernake Forest on the south-east of that town. Near the former place they cover a valley almost a mile in length as thickly as sheep grazing in a flock (hence their name of greywethers), and some of the largest of them have been employed in the druidical temple of Abury, at the head of this valley, whilst Savernake Forest has probably supplied the gigantic masses used to form the pillars of the large circle at Stonehenge. The valleys called Clatform Bottom, Fifefield Bottom and Lockridge Dame, near the villages of Kennet and Overton, are also strewn abundantly with blocks of the same kind; but they are rapidly diminishing, from the custom which has of late been extensively practised of splitting them with a heavy hammer after being gently heated. This practice has been even extended to some of the masses that once formed part of the temple of Abury. . . . Similar blocks of sandstone are also found scattered in great abundance over the chalk valleys at Ashdown Park on the south-west of Wantage."

Mantell, in his *Geology of the South-east of England*, speaks thus of these curious stones: "Immense blocks and boulders of siliceous sandstone, composed of granular quartz, and occasionally enveloping chalk flints and other extraneous bodies, lie scattered over the downs, and on the ploughed lands near Brighton, Falmer and other places. This sandstone is perfectly analogous to that which occurs in Berkshire and Wiltshire. The cement of the beautiful conglomerate or pudding-stone of Hertfordshire agrees in its character with the druid sandstone, and from that breccia also occurring in detached blocks above the chalk it is now generally supposed that they are both of contemporaneous origin; the siliceous deposition, when it did not envelope any foreign substance, forming the rock called the greywethers, and, when

it fell among pebbles of any kind, composing a breccia or *pudding-stone.*"

"The pudding-stone is exceedingly rare in Sussex, but specimens sometimes occur; and several examples have been found in the vicinity of Newhaven that could not be distinguished from the Hertfordshire breccia.

"Examples of the siliceous sandstone may be seen on the hill near Lewes racecourse, at Bormer, in Stanmer Park and on the ploughed lands near Hogsgrove farm. At Falmer the pond that supplies the village with water is surrounded with large blocks, and there are a considerable number in Stanmer Park, the seat of the Earl of Chichester, and also around Brighton. These boulders are of various sizes, some of them exceeding nine feet in length; their colour is either white or of different shades of grey and reddish-brown; their texture is subcrystalline; the whole varieties when recently broken much resembling lump sugar. In a few instances they enclose chalk flints slightly worn and small fragments of a dark green substance, the nature of which is unknown."[1]

Boulders of druid sandstone also occur in the shingle bed and calcareous deposit at Brighton, and may be observed lying on the sea shore in considerable numbers after a recent fall of the cliffs. Upon comparing the sandstone of Stonehenge with that of Sussex no perceptible difference can be detected; and in this county as well as in Wiltshire it has been employed by the earlier inhabitants as landmarks to denote the boundaries of towns[2] and villages or to commemorate the site of battles; as sepulchral stones to perpetuate the memory of their chiefs, and as altars on which to sacrifice to their gods. No regular stratum of the druid sandstone has yet been discovered in this country, and its geological position is still undetermined (*op. cit.*, pp. 48-50).

Prof. Rupert Jones says of the Sarsens: "These large stones are spread about the country, especially in Kent,

[1] Prof. Rupert Jones suggests that it may be glauconite.

[2] "The frequent occurrence of large smooth blocks of stone on the boundary line of villages and parishes in the south-east part of Sussex must have been noticed by many of my readers. A large boulder of druid sandstone placed at the corner of Ireland's Lane, in St. Anne's parish, forms the western boundary of the borough of Lewes. Similar stones are not unfrequent in the large tumuli on the downs; several may be seen near Lewes racecourse."

Surrey, Berks and Wilts. They lie thickly in some places, especially a little to the west of Marlborough, and are thinly scattered elsewhere among the heather of the downs and the grassy herbage of low grounds or peeping out on arable fields. They often lie along hedgerows, where they have been hauled and left by the farmer. Many have been buried out of the way of the plough. Hundreds were formerly to be seen about the country in cromlechs, standing stones and ancient circles; but of these comparatively few now remain. Not unfrequently Sarsen stones are found in the gravel beds of the surface. Being the only durable stones in some districts, innumerable blocks have been used as stepping stones in brooks and wet lanes; as border stones for ponds; as corner stones along roads and village streets; as foundation stones in churches, bridges, houses, barns and outbuildings; as building stones in large and small edifices, castles, churches, houses, cottages and various walls. Enormous quantities have been broken up for making and mending roads, also for paving and gate posts. The art of breaking and destroying the very largest has been long known and freely used, namely, by lighting narrow streaks of firewood across a block and pouring cold water on the heated lines, and then bringing the sledge-hammer into play on the pieces. So also a line of shallow pits chipped across the surface gives a line of weakness for breakage.

"When exposed on the downs these stones are often grey with lichen, and their own colour varies from brown to a yellowish tint and grey. In shape they are more or less quadrangular, longer than broad, and much broader than thick. They may be plain and smooth or undulating and irregular, with hollows on the surface; one face is usually flatter than the other. They often occur broken in two with a sharp, clean, straight fracture across their length."

I will now refer to some of the most remarkable of these stones described by Prof. Jones.

On the north side of the Kennet, in a valley near Marlborough, is a conspicuous block thirteen feet long, ten feet broad, seven feet thick, containing about 850 cubic feet and weighing about fifty-seven tons. "This looks like a small hut at a distance" (Whitaker). At Avebury (Aubury or

Abury), in the great circle, is one of these stones sixteen feet high, and estimated to weigh sixty-three tons. One in the "Kennet Avenue" is twelve feet high, six and a half feet broad, and three and a half feet thick.

Lord Pembroke, Stukeley tells us, estimated "the general weight" of the stones at Avebury "at about fifty tun," and some were thought to be seventy tons (Dr. Stephen Hale). One of the small circular sets is said to have had a stone twenty-one feet high. In a commonplace book of Dr. Stukeley we read "west of Abury is another entrenchment set with stones, one whereof makes the end of a barn".

At Stonehenge the outer Sarsens mostly stand twelve feet seven inches out of the ground, and are about six feet broad and three feet six inches thick, about 273 cubic feet in contents and seventeen tons in weight. The imposts are about ten feet long, three feet six inches broad, and two feet eight inches thick. The highest stone at Stonehenge is computed to measure under twenty-five feet, while a large stone which stood at Avebury a few years since is said to have weighed ninety tons.

The famous cromlech in Kent, known as "Kit's Coty House," is a Sarsen; so is the well-known "Blowing Stone" at Kingston Lisle, at the foot of Uffington Downs, in Berkshire; so is the "King's Stone," or "Coronation Stone," at Kingston-on-Thames, on which it is traditionally said so many of the Anglo-Saxon kings were crowned. Mantell tells us how these boulders were once used as sepulchral stones. Beneath one, near Brighton Church, an urn with human ashes was found by the Rev. J. Douglas, F.S.A.

An immense block of this kind is situated in Hove parish, near the Shoreham road, and is vulgarly called "The Goldstone," from the British *col*, or holy stone. Near is a dilapidated cirque of the same kind of stone, and on a farm opposite are two dilapidated kistvaens formed of similar materials, and on each side of the British trackway leading to the Devil's Dyke blocks of the same kind are to be seen (Mantell, *Geol. of S.E. England*, p. 60, note).

The variety of material of which they are constructed, in some instances worn or unworn flints, in others pebbles, forming pudding-stone, in others, again, fine ferruginous

grit, shows that almost any material formed the rough substance of the stones when brought in contact with the cementing material. This agglutinating matter was very pure silica.

The cement of the beautiful pudding-stone of Hertfordshire agrees in its character with the druid sandstone, and it is agreed they are contemporaneous. When the siliceous deposit did not envelop any other substance it formed greywether sandstone and, when it fell among pebbles, pudding-stone. Pudding-stone is very rare in Sussex, but it has occurred near Newhaven quite like that of Hertfordshire, with all the edges rounded. Some occurs in the shingle bed at Brighton, and may be seen on the shore after a recent fall of the cliff (Mantell, *Geol. of S.E. England*, p. 48).

In regard to the flinty conglomerate of which some of the Sarsen stones are formed, we are told in the catalogue of rock specimens in the Museum of Practical Geology that in many instances the same block furnishes an example of both structures, one portion consisting of sandstone and another of conglomerate, occurring with a well-defined line of separation between them. It has been remarked by Mr. Aveline that the majority of the Sarsens about Lambourne have a thickness of about two feet, as if they had come from one and the same bed of sandstone. He says he noticed a similar correspondence in the general thickness of the stones, many hundreds in number, covering the bottom of one of the valleys to the west of Marlborough.

Mr. A. C. Smith says: "Many persons from seeing these Sarsens conjugated in the lower part of the valley, as in the bed of a stream, conclude that they belong exclusively to the valleys; but this is a mistake. To see them in great abundance one should visit the hills above, where large districts of the downs are thickly studded by them" (*Arch. of W. Wilts*, p. 134).

Let us now turn to the question of the origin and age of these stones. Unfortunately, as I have said, none of them contain any fossil remains except roots of trees, the species of which is not distinguishable, and we have to decide the matter on other grounds. The question has been examined at great length and with much skill by Prof. Prestwich, F.R.S., Mr.

W. Whitaker, F.G.S., Sir A. C. Ramsay, F.R.S., Mr. Aveline, Prof. Rupert Jones, Mr. O. Fisher and others.

Mr. Osmond Fisher, speaking of some of the Sarsens which are found near the village of Broadmayne, five miles from Dorchester, tells us they are there found embedded in a fine sharp white sand which envelops them. There they are evidently *in situ*, and illustrate the position that they are merely the hardened concretionary portions of the sand bed. Where we find them uncovered and bare and lying about on the downs they have clearly been washed out of this sandy matrix by some denuding force. Everyone now seems to agree that the greywethers are derived from eocene beds which have been denuded and removed, and have left their harder concretionary parts behind in this way. The only polemic which has arisen has been as to what particular horizon in the eocene series they represent, some authorities favouring the theory that they have been derived from the Bagshot sands of the middle, and others from the Reading and Woolwich beds of the lower, eocene beds. Others, again, as it seems most reasonably, derive them from both these series of beds, for both in the Reading beds and in the Bagshot series we find sands which have in certain places been consolidated into concretionary masses by the infiltration of silica, and in both series also there seem to be pebbly puddingstones as well as sandstones. As Prof. Jones urges, the eocene beds thin out as we travel westwards, and not far west of Marlborough Forest the Woolwich and Reading beds as well as the London clay and its basement bed all disappear, and consequently the Bagshot series must have lain directly on the chalk just where the greywethers occur in the greatest number. Hence these blocks near Clatford are most probably the concretionary sandstones of the Bagshot sands. In Berks the Sarsens for the most part were also derived from the Bagshot beds, though some may have come from the outcropping edges of denuded Reading beds. In Surrey the Bagshot series were surely their source, for they lie on surfaces 200 feet and more above the Reading beds below. In Kent, Middlesex and Hertfordshire the Woolwich and Reading beds were the more prolific sources of these blocks. Sarsens are very often found in the patches and

pockets of sand, brick-earth and gravel on the chalk (as near Wycombe, Nobles, Napple Common, Walter's Ash, Denman Hill, Bryant's Bottom, Hampden Row, etc.), also in the Marlborough railway cutting at Inkpen and many other places. Sarsens are particularly abundant in the gravel of the Kennet valley near Newbury, and are usually then much eroded and worn. "These materials are the remnants of tertiary beds once lying, perhaps thick, on the chalk, together with some of the flint of the denuded chalk itself. On Barbury Down (and probably elsewhere) occasionally a green-coated flint, peculiar to the lowest stratum of the Woolwich and Reading beds, may be picked up, showing that these beds contributed some of the alluvial spoil of that region, and possibly, as far as they reached, some of the Sarsens" (Jones, *Wilts. Mag.*, xxiii., p. 133).

Prestwich derives the Sarsens from the mottled clays and sands overlying the chalk known as the plastic clay and from the Woolwich and Reading series, the sand in the blocks being precisely the same as the sand in those beds.

Whitaker attributed the blocks lying on the downs west of Marlborough to the Bagshot sands, these sands having once covered the chalk there where they extend beyond the London clay. In further evidence he says he had found some of the blocks resting on the London clay which intervenes between the Bagshot series and the other beds. The Sarsens abound on this outside edge. They contain no fossils, whereas the underlying beds are sometimes fossiliferous. He agreed with Prestwich, however, that some of them came from the plastic clay.

The question of the exact horizon from which the Sarsen stones were derived is, after all, an unimportant matter for us. It is enough that they formed part of eocene beds, and that, as the geological surveyors say, "In their present disjointed state it is clear that they are only the fragments of a stratum which had a very wide range, and which there is every reason to believe, along with other eocene strata, spread over (what are now) the chalk downs of the west of England". Outliers and other portions of lower eocene beds preserved in pot-holes in different parts of the chalk district, sometimes occurring on the very verge of the chalk escarp-

ment, also prove that these strata once extended over broad areas of the chalk.

A more important matter for us than the original *provenance* of these Sarsen stones is the fact, almost universally admitted by those who have examined them, that whether lying on the bare downs, or embedded in brick-earths and gravels, they are not *in situ*, but have been drifted away from their original position and been distributed irrespective of the contour of the country, *i.e.*, that they are, in fact, true erratics.

Mr. Osmond Fisher speaks of certain of these Sarsens which "have been carried by some torrential action into the deep valleys of Porksham and Bridehead". Prof. Rupert Jones speaks of Sarsens generally as "having been subjected to the wear and tear of water and shingle in the earlier time of the denudation of the parent bed, and afterwards possibly to the destructive action of blown sand when the block lay deserted by the water," and he attributes to the action of water the fragments of Sarsen stones often worn into bizarre forms, and frequently reduced to mere subangular stones in the various gravels of the country, the smaller fragments in the later gravels having been subjected to renewed water action again and again. "In both sets of gravels, both that of the plateaux and that of the valleys, we find numerous Sarsen stones. . . . They are broken and water-worn, but those in the low-level gravel to a much greater extent than those in the higher gravel. The breaking up of these masses . . . may have been due to frost (! ! ! H.H.H.) rather than violence, but the surface bears evidence of having been slowly worn by sand and pebbles washed over them persistently, worrying out cup-shaped hollows and tunnel-like holes, especially where small trumpet-shaped apertures of the tubes due to congenital root-marks, or the ends of small stems on fractured faces, presented depressions suitable for the erosive action of eddying sand and water." Again he says: "The streaming of the stones along the valleys, and their unequal distribution along their sides, suggest that the currents and tides which wore away the old tertiary sand beds had some influence (aided by prevalent winds, storms, and perhaps by the floating ice of a frigid climate) in shifting the blocks themselves,

and leaving them more in the hollows than on the hills." Again: "Doubtless many blocks were forcibly moved away from their parent beds and knocked about as the strata were denuded at their edges by encroaching waters. In the gravel at Crawley or Portisbury Hill, above Camberley, a spur of the Frimley ridges, very fresh Sarsens are met with. The upper Bagshot sand has been denuded and replaced by the high level ferruginous flint gravel; and in this the Sarsens lie, their mother-sand having been removed from above and around them, but still almost in contact with their convex lower face. Sarsens are very often found in the patches and pockets of sand, brick-earth and gravel on the chalk, and usually are there much eroded and worn."

Dr. Joseph Stevens, in his paper on Sarsens in the *Transactions of the Wilts Society*, says: "Although some of the Sarsens are unworn and untravelled, others show signs of rough usage from swift currents of long duration, as they are found in places, as on Inkpen Common, drifted in amongst heterogeneous materials, derived from every formation in the London basin. . . . All these several conditions were not accomplished without much and diversified handling on the part of the many handmaids employed by nature."

Tertiary boulders closely allied to Sarsens occur beyond the district where the latter have been usually noticed. Thus Mr. R. Godwin-Austen, in describing the tertiary beds in Devonshire, speaks of a stratum of considerable thickness, composed chiefly of angular chalk flints with, in its lower part, large tabular and angular blocks of chert and sandstone mixed with sand, as found on Blackdown and its ramifications, resting occasionally on chalk, but oftener, as on Haldon and Milber Down, on greensand. It lies on a very uneven surface, as is seen on the coast east and west of Sidmouth; the depressions producing the rugged outlines being either troughs or deep inverted cones. In 1836 he says he examined one of the latter which had been emptied of flints. The stratum in which it occurred was a very compact sandstone, and the sides of the circular pit had deep concentric grooves, such as would be produced by the circular motion of the materials within. Similar pits have been noticed on the surface of the chalk, and are very common in that neighbourhood. They cannot be attributed

to the action of acidulated waters, as they are not confined to calcareous beds; they must be the result of the mechanical action of hard substances set in motion by water, as basins and pits are now forming in rapid rivers. This bed of purely cretaceous materials, the flints coated by a black substance and mixed with abundance of stoney clay, is very distinct from the overlying waterworn flints and pebbles of older rocks, but corresponds exactly to the lowest tertiary beds elsewhere when they rest on chalk. In the Bovey valley a similar bed underlies the pipe-clay beds. Other portions of the tertiary beds once extended here. Scattered largely over the surface of all this district, and mixed with the débris on the hills, are blocks of a breccia composed of angular fragments of chalk flints, cemented together by an exceedingly hard siliceous paste. Besides the breccia, there are large slabs composed partly of similar materials, and in part of a compact, fine-grained sandstone; some blocks containing only an occasional flint, but some none at all, in which case they are mineralogical greywether sandstones, and may probably be the equivalents of those siliceous masses, warranting a presumption that tertiary deposits once extended wherever this breccia now occurs, for the blocks are so angular that they cannot be supposed to have been conveyed from a distance. The breccia, with its accompanying sandstones, occurs on the Blackdowns. It is very common in the valleys about Sidmouth; it has been worn into rounded boulders and pebbles in the great valley of the Exe; it is found again in tabular masses on the Haldons, below the accumulation of more rounded materials, and in similar angular blocks beneath the pipe-clay. A very large block serves as a footbridge over a watercourse near Kingsteignton. (See, for similar blocks in Dorsetshire, *Geol. Trans.*, 2nd series, iv., p. 4; vi., pp. 448, 449.)

In two deep combes excavated in the chalk west and south of Blackdown Hill, *viz.*, at Bride Bottom on the west and Portisham on the south, these blocks of angular breccia are accumulated as thickly as the greywethers in Clatford Bottom, near Kennet on the Marlborough Downs. Their abundance in Bride Bottom has led to its being called the valley of stones. This bottom forms the upper extremity of the vale of Bredy, where it contracts into a deep and narrow combe, at the head

of which the blocks are spread over a space of several acres, like a flock of sheep, often so close together that a man may leap successively from one to another. On the south side of Blackdown is another collection of these huge blocks of the same breccia in a steep combe descending the chalk escarpment into the village of Portisham. In the village itself they lie so thick that they partly obstruct the street, and sometimes the walls of the houses are planted on them. In the street at West Lulworth similar blocks lie in the line of front of the cottages and are built into the walls. Kidd mentions a similar collection in Berkshire, near Ashdown Park, on the south of Swindon. Although these blocks have been entirely separated from the matrix in which they were formed, they are very slightly rolled, and have been drifted but a short distance from their native place (*Geol. Trans.*, 2nd series, vol. iv., pp. 5, 6).

This will suffice to show how widespread the Sarsens are, and how clear their claim is to rank with the chalk and lias and oolite boulders further north as true erratics.

Since the above was written Mr. Rupert Jones has published a most interesting supplement to his monograph on Sarsen stones, containing many interesting details about them, none of which, however, affect the arguments above used (see Rupert Jones on Sarsens, *Geol. Mag.*, 1901, pp. 1-3, 54-59 and 115-125).

Having considered the erratics and other boulders of the south of England, which seem to me to necessitate our postulating that whatever distributed the similar boulders in the north also played a part in distributing them, let us now turn to the gravels in the same area. These, again, have in many places the character of true drift, and are formed partially of far-travelled pebbles.

Mr. N. Whitley, in describing the so-called raised beach and cliff boulders in Falmouth harbour and the drift beds on Plymouth Hoe, first speaks of the beds at Falmouth, of which he says: "The whole of these beds are unlike in their structure and materials to those of a sea beach: they contain no sea-shells or corals or relics of the sea; on the contrary, the sand is similar to river sand, and the upper bed in which the long pebbles and fractured stones are pitched upright in the

loam is similar to that exposed in inland sections, and more particularly to the 'head' over glacial deposits." In regard to the beds at Plymouth he describes one bed as containing "boulders and pebbles in a confused matrix of sand and clay, the stones mainly quartz, with others of blue grit, various granitoid rocks and pieces of limestone, and also of patches of limestone, rubble and clay, with patches of white and red siliceous sand, the grains of which under the microscope are rounded. . . . The beds show no horizontal bedding, but lie in patches on the upturned broken edges of the limestone beds, and in the cavities and fissures of the limestone. . . . In the section of gravel and clay large and small pebbles, mixed with sand and finely pulverised clay, have been carried on together, and in some parts the mass appears to have been a contorted semi-fluid slush." Dr. Moore tells us that a few shells (*Patella* and *Buccinum*) have been found in these beds at Plymouth; and recently in their upper part, ten feet below the surface of the present soil, there were discovered bones, teeth of elephant, rhinoceros, bear, horse and deer, the caudal vertebræ of a whale, and the lower valve of a large oyster—proving their derivative character.

It seems clear from this account that these beds are, as Mr. Whitley urged, really drift beds, and not raised beaches at all. The pebbles, as he says, are mainly derived from the district north of Plymouth.

Mr. Austen cites the fact of the abundance of chalk flints uninjured by transport or attrition which crown the greensand hills of Haldon, and the fact that the high lands along the coast from the Exe westward present a very uniform elevation (the dip of the beds being in the same direction and at such angles that, proceeding from west to east, we encounter a constantly ascending series, and find the various divisions smoothed off as they rise to the general surface line of the country), as evidence that the chalk formerly extended over portions of South Devon. The greensand has its surface furrowed, on which surface is a thick capping of débris (*Geol. Trans.*, 2nd series, vol. vi., pp. 449-451).

At Petrockstow, in the centre of Dartmoor, is an isolated bed of Dartmoor gravel which has travelled twelve miles from its source. This has been identified with the so-called glacial

gravel of Bovey Tracey. Many of the flints in Devonshire (at Newton Bushel, near Torquay, the gravel is 100 feet above the sea), as well as some greensand gravel on the opposite side of the valley, are apparently relics of a mass which has floated from the north (*Geol. Mag.*, 1878, pp. 113-115).

"In Devonshire and Dorsetshire the chalk with flints," says Mr. Horace Woodward, "is in many places associated with deposits of chert detritus, the fragments being sometimes rolled and transported to a distance from the spots where they were originally formed. The flints from the chalk also are sometimes rolled and transported to positions where they could not have been worked out *in situ*. Mingled with these accumulations we find, near Yarcombe and other places between Honiton and Chard, many quartz pebbles and pebbles of hard quartzose grit and quartzite which are evidently foreign to the immediate neighbourhood." Mr. Woodward urges that, if not relics of tertiary deposits, they may have been introduced during the glacial period. "Near Tiverton," he adds, "there are also considerable accumulations of gravel which may be of glacial age."

In regard to the gravels and sands at Bovey Tracey, the same author says: "The deposits consist of coarse gravel and sand with seams of clay, and the stones include large blocks and pebbles of grit, quartz, flint and chert". Mr. Pengelly has described some scratched stones found at Englebourne, and some boulders of hard micaceous sandstone met with at Waddeton, near Dartmouth in South Devon, which he argues are suggestive of glacial action, although the boulders are of local origin. The same is the case in North Devon, at Saunton, and other places where boulders have been observed.

Mr. Whitley says: "Other pebbles on the Hoe (*i.e.*, of Plymouth) are similar in structure to the fine-grained quartzite exposed in Cann quarry, and the most perfectly rounded pebbles are composed of hard black hornblende, derived from the border of the Dartmoor granite. These, with waterworn and angular pieces of chalk flints, are found both in the gravel beds and deep down in the fissures of the limestone. . . . But from whence came the chalk flints which are found both in the diluvial gravel and in the loam which fills the fissures of the limestone to a depth of at least sixty feet?

The old surmise that they were brought to our shores as ballast cannot be admitted here. Similar pebbles and flakes of flint have lately been found by Mr. Francis Burt, F.S.A., on Sladdon Heights, on Maker Hills, in the neighbourhood of Plymouth, and on many of the highest tors of Dartmoor, and their wide distribution over the northern parts of Devon and Cornwall has lately been established. Nor can we stop here; the trail of these fractured flints can now be traced northwards along the Welsh coast, over the Isle of Man, and up to the Mull of Galloway in Scotland, and also up the eastern coast of Ireland from Ballytother Bay, near Cork, to their native home and birthplace in Down and Antrim."

In regard to these flints, and speaking of the ancient beach near Braunton, Prof. Hughes says: "An examination of the flints themselves will, I think, be sufficient to justify our dismissing several hypotheses suggested as to their origin and mode of transport. They are not flints of the kind used by primeval man for the manufacture of implements.

"Their numbers, wide distribution and constant association with ancient deposits at various levels make the hypothesis that they were accidentally imported quite untenable. They go back to times far earlier than any ships that carry ballast. They are irregularly iron-stained, subangular gravel stones, not flints derived directly from the chalk.

"They are of the same kind as those common in the high level marine deposits of Wales (see *Quart. Journ. Geol. Soc.*, xciii., p. 83) and the north of England, and occur all round our western coasts; they are found on the St. David's plateau; they have been brought to me by Mr. F. J. H. Jenkinson from the gravels that occur here and there all over the Scilly Isles; they form part of the great gravel banks of unknown age that lie off our south-west coast." Prof. Hughes identifies them in date with the well-known Moel Tryfaen and St. Asaph gravels of North Wales (*Quart. Journ. Geol. Soc.*, 1887, pp. 662, 663).

In regard to the flints in the Scilly Isles, Mr. Nicholas Whitley says: "These islands are wholly composed of granite, being, in fact, the hilltops of a large granitic boss, only the highest parts of which are above the level of the

sea". They are remarkable for having abundantly scattered over their surface numerous fractured flints, especially in the north part of the island of Tresco, but becoming fewer on ascending the slopes of the hills. On the highest part of the island, about 140 feet above the sea, only a few pieces of scattered flint are found. The cross section of the island shows flint flakes embedded in the soil from one to two feet deep, and some as deep as eight feet, where a few large flints, with waterworn boulders of trap, hornblende rock, slate and grit (all rocks foreign to the district) are mixed with the local detritus of the islands. Mr. Whitley also found the flint flakes in the islands of Bryer, St. Helen's, Tean and St. Agnes. They could not have been brought in chalk ballast and spread over the land for manure, as Sir Henry De La Bêche has suggested of flints in Cornwall, for they are seen imbedded in contorted strata of drift in the section of the cliffs, and spread from shore to shore on unfertile crofts, and he suggests that with the other foreign rocks they are true drift (Whitley, *Flint Flakes*, etc., pp. 57-59). We may, perhaps, carry this particular line of drift further.

In a paper by Mr. J. A. Birds, on the geology of the Channel Isles, he speaks of much of the surface in Jersey and Guernsey as being covered with clay and sand, while chalk flints occur on the coast. In the south-east of Guernsey the deposit he says resembles genuine drift, and is sometimes 200 and 300 feet from the sea.

Let us now turn elsewhere. Mr. Horace Woodward says that Mr. Lucy has described the drift gravel of the Cotteswolds as containing fragments of greensand, chalk and chalk flint in addition to more abundant fragments of older rocks. Thus an isolated hill at Limbury, near Hartpury, in the vale of Gloucester, showed eight feet of gravel, composed for the most part of pebbles of quartzite, with fragments of silurian and other strata, besides eruptive rocks, with, *inter alia*, rocks from the north of England. On the Mendips, says Mr. H. Woodward, are deposits of loam and clay, with here and there a boulder of some local rock, whether old red sandstone or millstone grit, whose position cannot well be accounted for by the action of rain, rivers or the sea. Near Watchet and Mine-

head he confesses that possible evidences of glacial action have been described.

Turning elsewhere to another form of drift gravel, Dr. Kidd calls attention to the existence in the beds of gravel upon which Oxford stands, of a large number of local pebbles, but mixed with these are found pebbles of quartz and of various rocks, none of which is known to occur within a distance of fifty miles *in situ;* and frequently both the size of these foreign fragments is larger, and their degree of hardness greater, than those from the native strata of the neighbourhood. Thus he says at Bugley Wood and on a part of Wytham Hill, two or three miles to the west of Oxford, there is a coarse gravel at a considerable height above the level of the surrounding country, many of the masses of which are each of several pounds weight, and apparently derived from a district abounding in compact quartz rock, fine-grained micaceous gritstones, and obscurely defined compounds of hornblende and felspar, which last often assumes a porphyritic character. This coarse gravel contains few, if any, fragments of the neighbouring strata, and is altogether of a character so distinct from the gravel of the plain below that it is difficult to suppose the two are parts of the same bed. In travelling from Oxford the gravel changes with the subjacent strata, being chiefly in each district made up of the débris of the strata found there. He says he had made the same observation in Devonshire and in South Wales (*op. cit.*, pp. 151-153).

In 1853 Trimmer argued that three sets of gravels south of the Thames, at Horton Hill, Dartford and Rochester, were proved to be of northern origin by their contents, among which he specifies the Lickey quartzite pebbles; and he regards them as contemporaneous with the rolled gravels and upper erratics of the country north of the Thames.

Speaking of the dispersal of this gravel with quartzite pebbles, Buckland says: "The phenomena are in perfect unison with all the other cases I have been examining, and show the effect of a violent rush of waters from the north, which has drifted pebbles of quartz rock from the plains of Warwickshire and other central counties over the whole country intermediate between them and London, and has

mixed them up in each district with the angular and slightly rolled detritus of the adjacent hills, so that we have pebbles of the porphyry and greenstone of Charnwood Forest at Abingdon and Oxford, and pebbles of the rocks near Birmingham at Henley and Maidenhead and in Hyde Park" (*op. cit.*, pp. 198, 199).

These quartzite pebbles have since been traced very widely in the gravels and surface beds of south-eastern England, as far even as Essex and Suffolk; and it is clear they have spread sporadically from Lickey Hill, where the triassic Bunter conglomerates occur whence they were derived; and as the pebbles have been distributed quite independently of the river valleys, they form a test-mark showing that the gravels in which they occur are drift gravels, and clearly correlated with the other drift beds.

Let us now turn to the so-called hill or plateau gravels, the southern drift of Prof. Prestwich.

It is the fashion with those who have chiefly written on the southern gravels, and whose papers are monuments of patient work, to draw a more or less sharp distinction between the gravels which occur on the plateaux, the so-called high-level gravels, and those which occur lower down, which they style valley or low-level gravels.

The two kinds of gravel they discriminate and separate on the ground of their position, the two being declared to be of different ages, because they are at different levels, and, because of those at a low level being supposed to have been deposited by rivers, they cannot well have been laid down contemporaneously.

Mr. Whitaker remarks that many of the masses of gravel in the district adjoining London on the west are coloured as glacial on the drift maps only as a matter of probability, and admits that some may possibly be only very high terrace river gravel. Mr. Shrubsole, also, in his paper on the valley gravels about Reading, notes that there is a difficulty in making out a clear line of demarcation between the river and the plateau (*i.e.*, glacial gravel), and Mr. Monckton has stated that he thinks it probable that the higher terraces of the river gravel were contemporaneous with the glacial gravel (*Proc. Geol. Assoc.*, 1895, p. 28).

Mr. Monckton admits that the division between the plateau gravels and the valley gravels is an arbitrary one. The same pebbles are found in each and the same Sarsen stones. When the character of the one changes the other does so too, and except for the fact that they are found at different levels, and therefore create a certain *a priori* difficulty for those committed to certain fixed views on uniformity, I see no reason whatever for separating them. Nor do I see any reason for making the valley gravels derivative from and subsidiary to the plateau gravels, as Mr. Monckton does, when he says that "a very large portion of the valley gravels consist of materials mainly derived from the southern drift". They seem to me to be of the same date, laid down together by the same widespread impulse, and having nothing to do with river action at all. Upon this question I shall have more to say presently.

Mr. White remarks on the fact that flint implements have been met with in both sets of gravels, and adds that the lower spreads of the glacial gravel are so closely related to the river gravels as not to be logically separated from them, while their connection with the higher members of their own division is certainly no less obvious (*Proc. Geol. Assoc.*, xiv., p. 11, etc.). In regard to the changes in the composition of the southern gravels, Mr. Whitaker remarks how the sudden way in which a gravel that contains a large number of northern stones gives way to another that contains hardly any such, but a large number of southern ones, is remarkable, especially as the two gravels occupy like positions at like levels (*Quart. Journ. Geol. Soc.*, xlviii., p. 46). This shows how difficult it is to rely upon the character of the stony contents of gravels which have been obviously rearranged and drifted, and therefore have their contents mixed, as tests of age and horizon.

For the present, then, I propose to unite the high and the low level gravels in one class, and to give it Prestwich's name of southern drift. The distinctive features of this southern drift, as defined by Mr. Monckton, who knows it well, are that it consists mainly of materials derived from the Wealden area of Kent and Surrey, the chalk country to the north and west of the Weald, the eocene formations of the neighbourhood and older gravels, or possibly older "clay with flints" and surface débris.

Mr. Monckton divides them into three classes: the first consisting of certain patches on very high ground along the margin of the Wealden area itself; the second of material derived from the lower greensand of the Wealden area and extending as far as Strathfieldsaye; while in the third, which lies west of that place, there is an absence of such materials.

That is to say, Mr. Monckton unites under the name of southern drift, and most reasonably, as I think, gravels whose composition is not quite uniform, but dependent upon the distribution of the nether beds from which their materials have been derived. The southern drift he generally identifies with the hill gravel of uncertain age, coloured red by the geological surveyors.

While I cannot understand the rationale of separating the high-level and low-level gravels from each other, except on arbitrary grounds, I can as little understand on what principle Mr. Monckton separates the southern drift, or so-called plateau gravels of the south of England, in date from those gravels he calls glacial, that is, the gravels with quartzites. Like the former, the so-called glacial gravel is also essentially a plateau gravel, and its mode of distribution is precisely like the other gravel; like it, also, it consists in the main of flint pebbles. Its only distinguishing mark is that it contains a varying proportion of red and grey quartzites, weighing sometimes as much as half a pound, while the so-called plateau gravels do not; just as the boulder clay of Norfolk contains chalk boulders, while the boulder clay of Lancashire does not.

Why the plateau gravels west of Strathfieldsaye, which contain neither chert nor quartz pebbles, should be classed as of the same period and the same kind as those further east, which contain those stones, while the gravels close by, only occurring to the north, should be deemed to be of an entirely different period, because they contain quartzites and do not contain so many angular flints, is very puzzling to me. I believe them to be precisely of the same age and to have been distributed by the same force; the only difference being that being in a different locality they drew their contents partially from another source.

I cannot again understand upon what theory these gravels of the south of England are attributed to fluviatile action.

There is no river in the south at this moment which seems to me capable of rolling a rough stone into a pebble or of distributing the pebbles when rolled. They are for the most part slow, sluggish rivers, which nowhere erode their beds, and which are nowhere running on the rocks themselves out of which the valleys have been carved, and the masses of soft deposit in which they flow and the gravel they contain in places are really older than themselves. If they shift gravel at all, it is only in times of flood, and then not very far. Not only so, but the lower terraces and plateaux which are discriminated by Mr. Monckton are themselves far out of the reach of these modern rivers—*a fortiori*, the higher plateau gravels. Mr. Monckton speaks of the latter as the gravels of old rivers which had little or no relation to our present river system, whilst he says "the gravels of the valleys, terraces and varied plateaux are the work of rivers having some, though often a distant, relation to those now in existence". I cannot understand what he means. It is assuredly impossible to believe that since these surface gravels were laid out, the country has been everywhere turned so topsy-turvy that the whole drainage has not only been revolutionised, but that rivers flowed where the very highest ground now is. The way in which the gravels are distributed shows very plainly that the contour of the country was then very much what it is now. They cap the hills and plateaux, but they are also found flanking the valleys in places; and it seems to me that they show very plainly that they were distributed after and not before the contour of the country was settled.

I hold very positively that these gravels with their associated sands and Sarsen stones are the disintegrated débris of great stretches of tertiary beds, whose disintegration was not the work of rivers but of some widespread and much more potent cause, which swept over the country entirely neglectful of its contour, and left the débris it carried along at different heights and levels. It was powerful enough not only to break up the surface beds, but to carry along the great Sarsen stones and to roll them. People who call in ice forget that these stones generally have their angles rounded, showing that they have not been only carried, but

rolled. These stones are many of them also broken, showing the violence with which they have been swept along. Thus, in a paper on Sarsen stones by Colonel Nicolls, he mentions several angular Sarsens near Southampton, and says of them: "None of the seven stones which I have seen and examined are in the least degree scratched, striated or rounded, and the tortoise-shaped one at Bishopstoke and the dromedary hump-shaped one at Bevais Castle have been separated from their parent rock by a clean, straight, fresh-looking fracture, on their greatest diameter, so as almost at once to suggest the action of very heavy ice (!!!) as the disrupting power by a heavy side blow, the Bishopstoke example affording but a few inches of height for leverage in the fracture to some two or three feet of length fracture." Such facts as these are assuredly quite inconsistent with the gravels having been distributed as we find them by rivers. These gravels are in fact spread over the country irrespective of its contour, in more or less continuous sheets, and have no relation to any present or possible river system, and they seem to me as clearly continuous in conditions with the drift beds containing northern erratics, on the one hand, and with the sheets of gravel and shingle which in Suffolk and Essex and Herts have been classed as Westleton beds, and which contain occasional pieces of chert and quartzite mixed with the flint shingle, on the other hand, as may be. The main difference between the two seems to me to be that while the plateau gravels are due in the main to the disintegration of Bagshot beds, the Westleton drift is due to the disintegration of Reading beds. If we turn from the Thames valley to the district further south we shall have the same story to tell. In his paper on the Hampshire and Isle of Wight gravels (*Quart. Journ. Geol. Soc.*, xxvi., p. 535), Codrington, writing in 1870, says: "Fragments of Sarsen or greywether sandstone are met with everywhere, and blocks of considerable size are found in the gravel of the cliffs between Southampton Water and Gosport and near Southampton" (*Quart. Journ. Geol. Soc.*, xxvi., p. 535). Some of the Sarsens occurring in the gravels and brick-earths of Surrey, Berks and Wilts, and not unusually, are from eighteen inches to two feet square (*Journ. Wilts. Arch. Soc.*, 1886, pp. 123-126).

This does not exhaust all the drift gravels of the south of

England. There still remains the so-called angular drift, or rubble drift of Prof. Prestwich, otherwise called "head" and "clay with flints".

Godwin-Austen, I believe, first used the term "head," and explained it as the term used by the clay diggers for the horizontal layers of sand in the upper and middle valleys and a coarser deposit, often thirty feet thick, along the north-east side of them. "It lies on the pipe-clay. The materials of this head are angular and flattened rather than rounded, while the banks of sand are like those found in estuaries. The bones of wild boar, ox and red deer are found in the superficial sands of the Bovey valley, and beds of peat often occur below the head. These bones are black and polished like those found in rivers, and not like those in breccias" (*ibid.*, pp. 439, 440).

I shall have a good deal to say of this rubble drift in a later chapter. At present I would limit myself to affirming the conclusion which I fancy is now almost universally held, that it is not the result of the diurnal disintegration of beds *in situ*; nor is it possible to conceive how it could be laid down by any river under any conditions, since its contents are not rolled but angular, and it is distributed in widespread masses over great areas quite independently of the valleys and hills. In regard to this conclusion, I will quote a sentence with which I quite agree from a shrewd and capable geologist: "I see," says Mackintosh, "that the Rev. O. Fisher, in your last number, has arrived at a conclusion in support of which I have been collecting facts during the last eighteen months, namely, that the superficial angular débris, earth and loam, from which our slopes and hills partly derive their smooth and rounded forms, is not principally a disintegration *in situ*, but has been *carried or driven along* by a simultaneous *widespread* agency" (*Geol. Mag.*, iii., p. 575).

The view that this drift is not fluviatile, but the result of a widespread and general cause, was the matured conclusion of the two geologists who devoted the greatest pains and time to its elucidation, namely, Murchison and Prestwich.

The erratics and gravels are not the only deposits south of the Thames which resemble the so-called glacial beds further north. Mr. J. Shaw was shown a deposit of what he believed to be boulder clay at Fremington, near Barnstaple, and he is

supported by the Rev. W. S. Symonds, who thinks it may be a glacial till like that at Bovey Tracey.

This deposit of brown potters' clay at Fremington is in places forty feet thick and contains boulders of igneous rocks. The terminal curvature of the slate in South Devon, again, has been attributed by some to the effect of ice. Certain accumulations of angular detritus and stony loam, termed "head," seen along the coasts of Cornwall and parts of Devon, are probably, says Woodward, the equivalents of glacial deposits elsewhere (*Geology of England and Wales*, p. 493).

Mr. C. W. Peach says he had been told by Dr. Croll that on the Cornish coast, near Dodman's Point, at a height of about sixty feet above the sea level, he found the rock surface striated and ice-polished.

Mr. W. C. Lucy, in the *Geological Magazine* for 1874, records finding evidence of glaciation in the form of rounded rocky knolls near Minehead in Somerset, and a bed of gravel and clay seventy feet thick, which he deemed to be boulder clay, and also mentions the occurrence near Portlock of a large mass of sandstone, well striated, only partially detached from the parent rock. Mr. H. B. Woodward, in the same number, mentions Mr. Ussher's discovery of what the workmen called "rum stuff" near Yarcombe in the Blackdown Hills of Devonshire, which on investigation proved to be boulder clay, and, further, that it was not an isolated patch, but occurred in several places. Mr. Moore, in a paper on the drift deposits of the Bath district, describes the rock surfaces as grooved with deep and long-continued furrows, similar to those usually found on glaciated rocks, and concludes that during the glacial period they were subjected to ice action (Croll, *Climate and Time*, pp. 463, 464).

These phenomena especially abound in North Devon.

Let us now say a few words about the loamy deposits, *i.e.*, the brick-earths, etc., in and south of the Thames valley, the equivalents of the *alluvions anciennes* of the French, which were once deemed to be river deposits *in situ*. I have in a previous chapter shown how entirely different they are to river deposits, and their contents and structure prove them to be anything but beds *in situ*. They are rearranged and drifted and have all the marks of an external impetus having acted upon them

that we find in the boulder clays. They are, in fact, boulder formations of another kind, containing numerous and large erratics, which, instead of being of primitive crystalline rocks, are of greywether sandstone, while most of the organic remains they contain are organic boulders. The presence of northern stones in a drift bed is, as we have seen, quite adventitious and accidental, and in no sense an ear-mark of a true drift.

Of these beds we read: "In the gravels and brick-earths of Surrey, Berks and Wilts waterworn Sarsens and more or less rounded fragments are of frequent occurrence".

This is assuredly only consistent with the view maintained by Dr. Joseph Stevens, in which he concludes that the drifting or removal of the Sarsens from the original sands was coeval with the formation (say deposition—H.H.H.) of the brick-earth on the high chalk tracts.

Mr. H. B. Woodward says that "in Hertfordshire the brick-earth and boulder clay apparently pass one into the other. There is no evidence that the brick-earth and gravel are newer than the main mass of boulder clay. . . . I found it difficult to draw a boundary line between the brick-earth and the occasional remnants of boulder clay. In any case the evidence strongly favours the view that the gravels and brick-earths (with greywethers) belong to the glacial period." He mentions a large block of Hertfordshire pudding-stone, six by four feet by one foot four inches and weighing one and a half ton, from the gravels of Suffolk (*Geol. Mag.*, 1891, p. 121). In this paper Mr. Woodward describes several greywethers from the Thames gravels. The brick-earth in some places near Hampden and Bradenham in Bucks is opened up for the sake of obtaining the greywethers, which are broken up and shaped into small paving-blocks largely used at Aylesbury and other places.

At Grays the fossiliferous beds are covered with false bedded sands and brown clays, in which no fossils have occurred. These false-bedded sands afford evidence of constant change, not only in the direction of the currents, but in the nature of the material deposited and the absence of organic remains. The Sarsen stones occur on the upper surface of a bed of disturbed chalk above the solid chalk (*Geol. Mag.*, iv., p. 63).

It is curious to note that in some places the gravels underlying the brick-earths contain no travelled stones, while the brick-earths themselves abound in them (*Geol. Mag.*, ix., p. 159).

Dr. Hicks says of these beds: "Belt has demonstrated by numerous sections the similarity in the character of the gravels and brick-earth in the Thames valley with the glacial drift found at Hendon, Finchley, Whetstone, etc., in Middlesex, and has given many reasons why he considers they should not be classed with ordinary river deposits. We examined many of the sections in these districts together, and found the glacial deposits mantling the hills and descending the slopes into the valleys in many places to the level of the higher Thames valley implement-bearing gravels. Since that time I have had opportunities of examining many fresh sections on the plateaux and along the slopes at Kingsbury, Hendon, Finchley, Whetstone, etc., with similar results, proving that the main features are due to the underlying irregular floor of London clay. Evidence is therefore constantly accumulating, tending to show that the high-level gravels and the overlying brick-earth, from having so much in common with the glacial drifts, must be considered as belonging to the glacial period. They are found on comparison to have little or nothing in common with the recent deposits in the Thames valley, and are altogether unlike any ordinary river accumulations. The occurrence of an unstratified deposit, containing large stones at the base of the gravels in the Thames valley, agrees so exactly with the conditions witnessed everywhere in the glacial drift at Hendon and Finchley that one is inclined to refer this deposit to a very early place in the glacial period. It is in reality all that remains of the lowest boulder clay, and, as it consists mainly of local materials, it may be considered the local till. In the Thames valley, as on the plateaux in Middlesex, the lowest deposits are covered by more or less stratified sands and gravels (the so-called middle sands and gravels), and upon the latter in the Thames valley the brick-earth is found occupying apparently the position of the highest boulder clay in the other areas. Similar conditions are to be witnessed in the valleys and plateaux of the adjoining counties. In Hertfordshire glacial deposits are

spread out over extensive areas, and in the main they resemble those which occur in Middlesex" (*Trans. Hert. Nat. Hist. Soc.*, v., p. 149, etc.).

In another paper on the mammoth remains found in Endsleigh Street, in London, Dr. Hicks describes the brown clay with "race" and derived shells, covering the gravels with mammoth remains in these London beds, as answering to the glacial beds. He says of this clay with "race" that it is very widespread, and is met with at different levels over most of the undulating plains of north-west Middlesex. At Finchley it underlies a great thickness of chalky boulder clay. The sand and gravels below the clay are of a bright yellow colour. Large flints and Sarsen stones are occasionally found in the gravel, as well as many rounded pebbles from the tertiary beds, but the majority of the flints are subangular. A few pebbles of quartz and quartzite are found, and also fragments derived from the lower greensand. "There appear," he continues, "to be few, if any, very far-travelled rocks such as are present in the uppermost boulder clay a few miles to the north" (*Quart. Journ. Geol. Soc.*, 1892, pp. 466-468).

Overlying the brick-earths in the Thames valley and the angular drift, and also capping the so-called raised beaches, is a fine levigated calcareous marly deposit, corresponding in position and texture with the continental loess, and, like it, distributed as a drift deposit, *i.e.*, independently of the contour of the country. To a form of this deposit Mr. O. Fisher has given the name of "trail". He describes it as a clayey gravel and sand containing phosphatic nodules, with pebbles of very hard rock, much rounded and often a foot in diameter. It contains no fossils and is unstratified; it is a mixture of white clay with certain débris from the northern gravels and the greensand. How were these ingredients brought together? As Mr. Fisher says, the trail occurs in places in which rivers have never run as in the bottoms of dry valleys. It seems to me to be only another and a last link in the chain of the various drifted beds which I have tried to describe; and which, although composed of varying materials, are bound together by one supreme fact, namely, that they have all been carried along and drifted from their original site probably at the same time, since they are related in a similar way to our great indices of

time, the organic remains. The general conclusion I would press from the English evidence is that the gravels and brick-earths of the southern English counties containing great flints and masses of Sarsens and organic boulders, such as we have described from the boulder clay in earlier chapters, together with the "head" and "trail," are as much true drift as the boulder clay itself. The great dominating feature of both sets of beds, which unites them in a common class, is the fact of their being more or less foreign to the actual locality where they are found, and not being the result of disintegration *in situ*, and that they have all been subject to movement more or less considerable, independent of the contour of the country.

Let us now traverse the English Channel, and shortly converge our attention upon the surface deposits on the other side. They are virtually a continuation of those on our own side of the water. The angular or rubble drift from both sides of the Channel has been described by Prestwich. The valleys and plateaux of Northern France, like our own, are occupied by similar gravels and brick-earths, containing great masses of Sarsen stone, palæolithic implements and the débris of the same extinct fauna, and are known to the French as *diluvium gris* and *diluvium rouge*.

A few words about the continental Sarsens maybe acceptable to some. Long ago Cuvier and Brongniart, in describing the valley of the Seine, spoke of the loams and gravels which carpet it, and described this mantle as from four to six metres thick, and as often containing blocks of sandstone and menhirs which had been transported by forces no longer active (*Mêms. de l'Institut*, 1810, pp. 234, 235). Such blocks are not found in the valley of the Marne (*ibid.*, p. 235).

In the banks of diluvium of Saint-Gilles, of Moulin-Quignon and of the Champ-de-Mars, which are contiguous and form one deposit, we often meet with erratic blocks of "*grès*" of a metre in diameter. These banks, at whose feet, thirty-three metres below, flows the Somme, are not dominated by a point whence they could have been washed down. "How came they here if they were not thrown up by an upheaval. We must conclude they were swept along by an impetuous torrent, and even before the valley was excavated, unless we believe

the Somme then flowed 100 feet higher and that it swept them into their present situation." They are found from one to two metres from the surface, and M. Boucher de Perthes tells us that, like the druidical monuments, they have been employed by the people living there for building material. These blocks are much oftener found on the heights than in the valleys. They are common at Saint-Gilles, Moulin-Quignon and the Champ-de-Mars, and very rare, if not quite unknown, at Menchecourt. At Moulin-Quignon and Saint-Gilles they do not occur in the lowest stratum, but generally at a depth of one or two metres below the humus, or in the upper beds, whence the bones and objects of human workmanship are absent.

Lyell, in describing the brick-earths of the Somme, speaks of them as containing masses of greywether sandstone several feet long and generally having their edges unworn. He argues, I think, erroneously, that when spherical they owe their shape to an originally concretionary structure and not to trituration in a river-bed. These French greywethers are accompanied by broken chalk of every size, from a fine powder up to fragments as large as a man's head. "Many of these fragments of chalk," says Dr. Andrews, "though soft enough to write with upon a blackboard, have preserved with absolute perfection the sharp angles and edges which they had at the time they were broken from the cretaceous strata. It does not seem possible," he adds, "that they could have been rolled 100 feet in the bed of a stream without losing their sharpness."

It is plain, in fact, that these French brick-earths, with their palæolithic stones, mammoth bones and Sarsen stones, are portions of the true drift which occurs in so many different ways, but all with a common dominating feature, namely, evidence of transport by other than normal methods.

The existence of these Sarsen stones in France is very interesting on other grounds, as it proves that a large mass of eocene strata has also been broken into fragments there, and that these have been dispersed in the gravels, leaving hardly any trace *in situ*. Similar blocks of greywether have been found in the gravels of the Southern Netherlands, and a large number of them in the Morvan district of France. (See Potier, *Bull. Soc. Géol. de France*, 1879, p. 838, etc.,

who shows that there too they are the débris of disintegrated eocene beds.)

As I have said, these blocks occur largely in the widespread deposit of brick-earth specifically called diluvium by the French writers, and which seems unmistakably on the same horizon as the English rubble drift and brick-earth. In the case of this diluvium, the loam is mingled with sharp-edged flints, while these are largely absent from the *limon* and the *terre à brique* properly so called; but there can be no doubt that the view generally held that all these loamy deposits are on one horizon, and belong to one epoch, is well founded. They are all marked by the presence of the same land shells, the bones of the mammoth and his companions, and, more important, by the more specialised works of primitive man, and their difference of texture can be easily explained.

The diluvium in France and in Belgium and the brick-earths in South Britain agree in this cardinal factor, that they are all separable into two well-marked forms: one stratum bearing all the marks of being largely *in situ* and rearranged there; and the second stratum being not only markedly disturbed, but also drifted.

In regard to the diluvium of the French writers, the fact has long been familiar. One form of it is known as *diluvium gris*, while the other one is called *diluvium rouge*. For a long time a dispute prevailed among French writers as to whether the two are essentially distinct, or whether the *diluvium rouge* is not an altered and sophisticated layer of the grey diluvium; its *rubification*, to introduce a French phrase of M. Vanden Broeck, being due to the infiltration of water containing acids. The best opinion now, and the one generally held, is that, although there is a certain difference between the two strata, the pebbles in the *diluvium rouge* being more rolled than in the other, while there is a greater scarcity in it of those mammalian remains which characterise so richly the *diluvium gris*, nevertheless there is a complete continuity between the two, and that they belong essentially to one horizon.

The *diluvium gris*, with its numerous remains of a land fauna and abundant land shells, represents the rearranged old land surface upon which the mammoths lived, which is

largely *in situ.* It is found almost entirely in the valleys where subaerial deposits are naturally developed on the largest scale, and where they are least disturbed, and not distributed over the high ground, where such deposits are naturally scarce, and where, when they occur, we generally postulate that they are not indigenous, but imported.

The *diluvium rouge*, on the other hand, is found not only covering the *diluvium gris*, but also distributed independently of the drainage and of the river courses, and covering the higher ground and the plateaux, where it often lies immediately on the subjacent rocks. In Picardy and Northern France and in Belgium we have similar conditions—in the valleys an undisturbed deposit called indifferently *diluvium gris* or *diluvium des vallées;* while superimposed upon it, and also stretching beyond its limits far away over hill and dale, exactly like the *diluvium rouge*, we have the mantle of surface loam called *limon de Hesbaye, limon des plateaux*, etc. M. d'Acy has written a most elaborate and learned essay on this deposit, entitled "Le Limon des Plateaux du Nord de la France". With the views he maintains so ingeniously, and with such a fund of evidence, I most completely agree. He shows that this "*limon*" is perfectly continuous with the *diluvium gris*, and that it corresponds precisely with the *diluvium rouge*. Meanwhile, I would reaffirm what I have already urged, that the loamy deposits of Western and Central Europe, where we can examine them, present two distinct forms of deposition. One, *in situ*, but disturbed, known as *diluvium gris, diluvium des vallées*, etc., equivalent to the lower brick-earths of the Thames valley. The other, the *diluvium rouge, limon des plateaux*, upland loam or trail, having all the appearance of being greatly disturbed and drifted.

In regard to the stratum *in situ*, I would confine myself to one argument only to show it has been disturbed, drawn from the discontinuous nature of the deposit. If there had not been some denuding agency at work on a large and wide scale since this stratum was deposited, we should assuredly have found it still remaining in a continuous layer in the valleys, but this is not so. Nothing is more familiar than the disjunct character of the patches of brick-earth which

are found in *the valleys* of South Britain. The same is the case with the *diluvium gris* in France. These patches and local seams are the mere remnant which has not been denuded. Let us now turn to the upper stratum. As I have said, this upper layer in France and Belgium, where it is known as *diluvium rouge* and *limon des plateaux*, is spread over the whole country from the bottoms of the valleys right over the plateaux, independent of the local drainage of the district. It is very widely distributed in France and Spain, and sometimes, like the loess, is found in deposits of considerable thickness. It has been found overlying the sands of the Landes near Bordeaux, and also in the neighbourhood of Madrid, in Spain, and extending over the plateau of New Castile. It is found at heights varying from forty metres, near Paris, to 660 metres near Madrid. Elsewhere it is also found at great heights. M. Tardy tells us he met with it in 1871 on the Col del' Eremo, on the colline of Turni, at a height of 600 metres. Afterwards he met with it on the high plateaux between Le Puy and Meude, and between the latter village and Saint-Flour, at points 800 metres high. He tells us further that it contains pebbles with their edges rubbed. This widely distributed deposit, occurring thus on high ground far beyond the reach of any possible fluviatile action, apart altogether from the river drainage of the country, can surely only be accounted for, like the similar occurrence of the loess, by some general and not a mere local cause. There is no pretence for invoking marine action, for there are no marine remains, and the French geologists who have examined the problem have been compelled to accept a similar explanation of it as that given for their other travelled drifts. This view explains at once why we should find diluvium containing no lime spread over large districts where limestone is the prevailing rock, and where that element would undoubtedly have been markedly present if the deposit had been indigenous, and the product of the mere disintegration of the subjacent strata by fluviatile or other action, and not transported bodily from some other locality like a true drift deposit.

As we leave the north-eastern parts of France and travel towards its central districts we shall find the diluvium, which is in most places covered with a layer of finely levigated loam-

like loess, becoming more and more loess-like and quite continuously.

Let us now turn elsewhere and try and trace the connection of the northern drift, which covers the larger part of Scandinavia and the North German plain, with the deposits round it on every side, and which are more or less sharply separated from it by orthodox writers on the so-called glacial period. Here, as in previous pages, I will quote the views of those who have established a claim to be heard on the matter.

The so-called glacial beds of North Germany consist of sands, gravels and clays, containing in places marine shells, and also erratics derived from different quarters. As we proceed southwards and westwards, the more crystalline and far-travelled erratics become less and less numerous. The notion that there is a hard and sharp line which limits and bounds these erratics is no longer possible to maintain.

In recent years M. Delvaux and others have traced the existence of pebbles of Scandinavian origin to the western part of Belgium and even over the French frontier. In Northern Brabant, also, Deluc long ago found pebbles of granite, while Winkler described a large foreign erratic from the same province. This last stone is granite, and, according to M. Winkler, weighs about 7,000 kilogrammes, and is buried in the earth not far from the old church at Oudenbosch. It was not always there, but was originally found between Oudenbosch and Oudgastel, and, having been mistaken for an aerolite, it was called "Dondersteen". It was moved about 1808. It was originally planted on the so-called Zand diluvium, which, in this district, is free from pebbles, etc. M. Lorié has found pebbles of granite, porphyry and diorite in the heaths south of Uden and Mill, villages situated to the north-east of the railway from Boxtel to Wesel, and others, on the way from Eindhaven to Bladel, in gravel, which have been carried as ballast from Arendonck, near Turnhout. He mentions that in the diluvial sand at Heymenberg, near Reenen, in the province of Utrecht, no pebbles are found, but only large erratics (*Annales de la Soc. Géol. de Belg.*, xiii., 1886, p. 3, etc.).

While the northern erratics gradually die out as we travel to the south-west from Oldenburg and Groningen, their

place is not taken as in England by boulders of secondary rocks, for there are no beds in this district whence such stones could be derived; but we merely have the sandy matrix remaining, which is the same in texture and appearance with that in which the erratics of Germany are enclosed, and which is largely derived from the disintegration of the Belgian crag beds. In regard to this the best observers are at one, and I will quote two or three of them.

M. Élie de Beaumont, speaking of the parts of Germany and the Netherlands bordering on the North Sea, says: "Ils sont des pays sablonneux, formés d'un sable quartzeux, légèrement argileux, dans une grande partie duquel se trouvent des fragments et des blocs de différentes pierres, par exemple, des silex de la craie, contenant souvent des pétrifactions et des *pierres primordiales* (granite, etc.). Ces sables, très répandus dans l'Allemagne septentrionale, portent le nom de *geest*, qui dans le langage vulgaire de ces contrées désigne ce terrain qu'on trouve dans le pays de Liège et de Juliers, dans le Brabant, la Gueldre, l'Ober-Yssel, la Westphalie et la Basse-Saxe, formant le sol des bruyères, et qui dans cet espace continu couvre les montagnes aussi bien que les plaines; c'est là le sol fondamental de ces contrées" (*Leçons de Géologie*, etc., i., p. 255). The *geest*, says M. d'Archiac, occupies also the southern part of Holland, and is conterminous with the Campinian sands that stretch into Belgium.

M. d'Archiac quotes and incorporates another passage to the same effect. "In the *geest*," he says, "the boulders of northern rocks, so common in the neighbourhood of Groningen, do not advance further south beyond Arnheim on the Rhine, although the sand in which they are enclosed at Groningen, and which is the prolongation of the sands of Westphalia, crosses the Rhine and extends as far as Maestricht and into 'The Campine'" (*Histoire des Progrès*, etc., ii., pp. 142, 143).

Dr. H. Vander Capelle says: "Les observations faites près des escarpements, sur la côte méridionale de la Frise, nous conduisent à conclure que *le sable à erratiques* (Geschiebesand), *la couverture d'erratiques* (Geschiebebestreuung) et le Zand *diluvium* doivent être réunis comme les produits d'une même action, ainsi que M. Berendt la démontre, de même que M. Martin qui a fixé l'attention sur ce qui s'est

passé dans notre pays" (*Bull. de la Société Belge de Géologie*. iii., p. 252).

According to Dewalque (*Prodrome d'une description Géologique de la Belgique*, p. 241) the Campinian sands extend south in Belgium as far as a line drawn from Dixmude to Maestricht, by way of Ypres, Courtrai, Oudenarde, Alost, Malines, Louvain and Hasselt. He concludes that they are the western prolongation of that great sheet of sand which stretches north into Holland and is prolonged through Northern Germany, along the borders of the Baltic. In other words, the "sable Campinien" forms part and parcel of the great northern drift (see Geikie, *Prehistoric Europe*, p. 507, note 1).

"It appears well established," says M. Morlot in his work on the geology of Belgium, "that the Campinian sands represent the zand-diluvium of the great Baltic plain as M. Winkler has shown" (*op. cit.*, p. 294).

Just as the Zand diluvium and the Campinian sands are conterminous and continuous, so are the latter apparently continuous with the loamy deposits of Belgium and Northern France.

Hesbaye, Brabant and Hainault are covered with a mantle of fertile loam known as *limon de Hesbaye*. It is described as yellow, sandy, stratified and calcareous at the base, and contains numerous shells of *Succinea oblonga*, *Pupa muscorum* and *Helix hispida*. In its upper part it becomes more plastic and adopts a deeper reddish-yellow colour. Its lower part ceases to be calcareous and loses its stratification. It covers all the undulations of the soil with its mantle, and is sometimes known as brick-earth, and is clearly formed of the finer and more argillaceous part, which, by the way, is here called "*ergeron*". This loam also covers the diluvium proper, of which I shall have more to say presently. In every way, therefore, it seems to answer to the loess of the German writers.

There can be very little doubt that it is a mere prolongation (on the same horizon, but constituted of different materials) of the Campinian sands, which, as M. Mourlon says, take exactly its place in Limburg and Lower Belgium. Dumont also treats the Campinian sands as of the same age as the *limon de Hesbaye* (*vide Geol. Mag.*, vii., p. 201).

Godwin-Austen says of the Campinian sands that they overlie a surface with *Elephas primigenius*. In like manner, he says, the loess (*limon de Hesbaye*) overlies the gravel beds in which the fragmentary remains of the great pachyderm fauna occur. Both are subsequent to the Ardennes pebble beds. The occurrence of these pebbles at the base of each suggests that these two accumulations are of the same age, and such, it seems, was Dumont's latest view (*Quart. Journ. Geol. Soc.*, xxii., p. 250).

M. Erens, in his *Recherches sur les Formations Diluviennes du Sud des Pays Bas*, tells us how, in 1881, Berendt (*Die Sande in Norddeutschen Tieflande und die diluviale Abschmelzperiode*) proved a real passage between the gravelly and the sandy diluvium, and declared the latter to be only a phase of the former. He says that towards their summit the gravelly deposits become finer and finer, and that the coarse gravel thus passes into a fine gravel, and this into sand. M. Erens tells us he shares the view of Berendt, and points out the uniformity of the phenomenon and its widespread character, which shows that the change in question has not been due to mere local efforts, but to widespread ones; and he gives similar lists of the animals whose remains have been found in the two sets of beds, to show that they are quite synchronous (*op. cit.*, p. 37). On the other hand, he says the sandy diluvium is clearly synchronous or isochronous with the loamy diluvium and the loess of Limburg. Hence he unites the two under one term, *i.e.*, *Diluvium sablo-limoneux*. Here, again, he calls attention to the similarity of the fauna in the two sets of beds, and especially to the existence of that test animal, the mammoth. Both the sandy and loamy diluvium have the same relations with the gravelly diluvium, fill up the spaces between stretches of the latter, and often overlie them. They both contain near their surface great erratics almost identical in nature, and were doubtless distributed by the same cause. On the surface of the sands there occur great crystalline Scandinavian erratics, *e.g.*, at Legge, Basel, Oudenbosch, Hoogstraeten, etc. At Fauquemont M. Ehrens says he found a Scandinavian "augengneiss" in the gravelly loam. He attributes the transport of these stones to floating ice at the end of the glacial period.

Turning to the loess, M. Erens gives several reasons in addition to those quoted in an earlier chapter against treating it as an æolian deposit. He describes the animal remains in it as inconsistent with such an origin. He also quotes in proof of the same conclusion what is more directly germane to the contention in this chapter, namely, the presence in the loess of the Low Countries of huge erratic blocks of different origin—quartzites from the Ardennes, white quartz from the Rhine, greywether blocks like those of Fontainebleau, which appear as if they had been rolled, flint from the district itself, conglomerates from Burnot, and an angular block of Scandinavian "augengneiss"; and he concludes thus: "Le diluvium du Sud des Pays Bas est unique avec des phases diverses. Cette continuité, cette unité de notre diluvium nous l'avons prouvée par quatre arguments: (1) Par la faune, qui subsiste dans les différentes phases diluviennes; (2) Par le passage réel que l'on observe dans ces phases différentes; (3) Par les courants, qui restent les mêmes dans ces différentes phases; (4) Par les roches cristallines, qui sont à peu près des mêmes provenances. . . . A un niveau fort variable, non déterminable et dépendant de la distance aux rivières et des hauteurs qu'occupent les gravières, on distingue dans celles-ci un mélange de roches d'origines bien diverses; ardennais, moséennes, rhénanes, belges, suédoises, norvégiennes, bretonnes et normandes. . . . *couvert dans le Nord par le diluvium Scandinave, au Sud par le loess et partout ailleurs par le diluvium sableux*" (*Bull. de la Société Belge de Géologie*, etc., v., pp. 14-42).

M. d'Archiac says that he regards the sandy and gravelly beds which he had traced as far as Maestricht (*vide supra*) as of the same age with the deposit containing rolled pebbles, erratic blocks and bones of the great mammals, which are found in the bottoms of the valleys as well as on their flanks and their dividing plateaux, and which extend from the Rhine to the English Channel, that is with the diluvium of most French writers. Of this diluvium, again, he says: "South of a line drawn from Maestricht to Ghent, *i.e.*, in Limburg, Southern Brabant, Flanders, Hainault, the provinces of Liège and Namur, and beyond, over the district forming the watershed of the Seine, the diluvium is composed towards the

bottom of partially rolled pebbles, more or less mixed with sand and clay and often containing the remains of extinct animals; while in its upper part it is formed of a mixed loam of sand and clay, yellow or brown, and sometimes calcareous, which the French have called *alluvion ancienne*." In the Rhine district, where the two divisions of the diluvium also exist, the upper one, he says, is called *lehm* or *loess* (D'Archiac, *op. cit.*, p. 143).

M. de Mercey urges that the lower part of the *limon* of the plateaux of the north of France, which contains angular flints and is known as *limon biéfeux* or *limon grossier*, is of glacial origin. This he argues from its containing angular flints, mixed with flint nodules, and also from the unequally sized grains out of which it is heterogeneously formed, in which it simulates boulder clay. The finer brick-earth (loess) which overlies this loam is due to the levigation of the latter. As in the case of districts covered with boulder clay, those covered with the *limon*, when made of chalk, are deeply channelled and excavated, and the *limon* itself is spread like a continuous mantle, which follows the undulations of the ground. This *limon biéfeux*, which M. de Mercey makes a true glacier mud, corresponds to the yellow clay with stones and blocks that M. Dupont has described as covering certain parts of Belgium (*Prehistoric Europe*, pp. 164, 165). I do not agree with M. de Mercey's main conclusion, but I do cordially agree with the continuity of conditions he has pointed out between this *limon biéfeux* and the deposits elsewhere to which a glacial origin has been assigned.

According to Mr. E. Van Broeck, the loams of Belgium are separated into three horizons: first, the stratified loam, which contains mammals' bones and which doubtless answers to our lower brick-earths; the unstratified loam, which covers it, which is friable and pulverulent, and which is no doubt merely loess, and to which the name *limon de Hesbaye* is really applicable; and, thirdly, above this again, a brown clayey loam which is called brick-earth, and which differs from the two former by the absence of calcareous elements and by the larger proportion of clay which it contains.

Mr. E. Van Broeck has shown that this last section, like the *diluvium rouge* of the French writers, is really the same

as the stratum below, only that it has been chemically altered by the infiltration of surface waters (see *Mémoire sur les phénomènes d'altération des dépôts*, etc., *Mém. Cour.*, etc., *Acad. R. Belg.*, xl, 1880). This reduces the Belgian loams to two series —a stratified and an unstratified. Mr. Van Broeck admits that if we are to attribute the distribution of the unstratified loam of Belgium to the agency of water it necessitates a quick transcendental condition of things. His words are that if this were fluviatile we should have to appeal to a "régime fluvial incompatible avec les conditions climatériques les plus hasardées que l'hypothèse pouvait mettre à notre disposition". He scouts the notion of gigantic rivers filling up the valleys with water, which would alone explain on the fluviatile theory the distribution of this loess on the plateaux and the flanks of the valleys; and he refers to the various notions which have suggested themselves to meet the difficulty, such as the barring of river-mouths by ice, and the consequent flooding of the country behind, the sudden melting of glaciers, etc., all of which he rejects.

The facts point, it seems to me, most unerringly to this loess of the Netherlands being what the other drift deposits are, a widespread mantle laid down by a uniform and widespread cause. Let us follow this loess, the latest and the highest of the drift deposits, a little further.

In the north of France "the loess, while retaining the character of a sandy calcareous loam, yet frequently becomes more or less argillaceous, and even passes into a regular brick-earth, or it may consist of a succession of alternate layers of brick-earth and calcareous loam, or loess properly so called". This grey loam, as we have seen, is called *diluvium gris* by the French. Its upper layers are often more disturbed and oxydised, and are known as *diluvium rouge*, and this is often on the plateaux covered by true loess. It is in the *diluvium gris* that the great bulk of the French palæolithic implements have occurred. They have been discovered *under* the loess in the neighbourhood of Spiennes, south-east of Mons—another proof of the close relation of the loess to the boulder clay, which similarly overlies the beds with palæolithic implements. In this behalf it is well to bring the loess into line with another deposit.

"Although," says Godwin-Austen, "the true Campinian sand

has never been found to contain animal remains of any kind, it overlies a surface with *Elephas primigenius*. . . . In like manner the loess overlies the gravel beds in which the fragmentary remains of the great pachyderm fauna occur. Both the Campinian sands and the loess have Ardennes quartz pebbles at their base. . . . This suggests that these two accumulations must be nearly of the same age; and such was Dupont's latest view. . . . The *sables de Campine* and the *limon de Hesbaye* have never been seen to overlap, they rather pass one into another" (*Quart. Journ. Geol. Soc.*, 1866, p. 247, etc.).

The loess in Germany as in France is often distributed in layers of a different consistency, the uppermost one being the finest in grain, showing there has been a process of sorting. In regard to the middle layers in Germany, Mr. J. Geikie says: "They are not true loess, but rather calcareous clays, containing not a few rounded and angular stones, chiefly flint; but quartz, granite and other varieties also occur, some of the fragments having evidently been derived from the so-called 'northern drift,' while others may have come from the Hartz and districts to the south or south-west. One fragment of red granite must have weighed over twenty pounds" (*Prehistoric Europe*, p. 148).

Just as the boulder clay alters its composition and character with that of the subjacent beds, and especially in the upper reaches of the valleys, so does the loess. Thus in that of the Neckar, near Tübingen, as Lyell noticed, it is mottled with red and green. This, as Mr. Geikie suggests, is evidently due to the fact that there it owes its origin in great part to the degradation of certain variegated red sandstones which are common in that neighbourhood (*ibid.*, p. 152).

Mr. J. Geikie has a further paragraph on this subject with which I most completely agree. He says of the origin of the various loamy deposits here referred to: "We cannot have a special explanation for the loess or *lehm* of each particular region. They evidently pertain to one and the same period, and must owe their origin to some widely acting cause or causes" (*Prehistoric Europe*, p. 234).

According to Prestwich, there are cogent reasons for believing the loam of the plateaux and upper slopes of the valleys

of Northern France to have been laid down contemporaneously with the high-level gravels. "I believe," says Geikie, "the same rule holds true for all the great river valleys of Europe" (*ibid.*, p. 235). Mr. J. Geikie attributes the loam and loess, in fact, to the melting of the great ice-sheet, the waters from which, he argues, carried along great masses of gravel and shingle as well as finer materials, and he especially quotes in this behalf the sporadic stones and boulders sometimes found in the loess.

An important feature in regard to the loess consists in the animal remains found in it. Prestwich says the organic remains of this loess are those of the quaternary *land fauna* living in the respective districts at the time of the inundation, and include in several instances the remains of man. It tells, therefore, the same tale as the angular rubble and head ("Possible Cause of the Tradition of a Flood," *Trans. Vict. Inst.*, l., 11th September).

This is quite true; but the whole truth is not quite disclosed in these words. It seems to me as plain as possible that the remains of living creatures in the loess, namely, of man, of the great mammals, and of the land and fresh-water molluscs, are mere derivative boulders, as they so often are in the boulder clays and the brick-earths. They in no sense tell us of a long period during which the loess was being deposited. That homogeneous deposit without stratification or layers has no long history. It bears all the marks of a rapidly or suddenly deposited mass of mud, thrown down at one *coup* and not deposited in a series of ages; and its organic contents are the remains of the animals living on the land surface over which it was driven, and on which it was deposited, and do not testify in any way to a loess *period*.

Turning from the organic contents of the loess to its connection with the other drift beds, we have seen how in Belgium and France it passes into them. Geikie says "there is one opinion upon which geologists are pretty generally agreed, namely, that the loess of the great valleys of Central Europe consists for the most part of glacial mud," *i.e.*, to use my phraseology, is contemporary with the northern drift.

The distribution of loess over Germany has been well described by Penck. It runs, as he says, from Belgium right

across Mid Germany through the Hartz, Thüringerwald, Erz and Riesengebirge, as far as the northern outliers of the Carpathians, where it loses itself in the great loess plains of Podolia and the South Russian steppes. Another band traverses South Germany. It abounds in the low Rhine valley between the Lake of Constance and Basle, but does not penetrate the Black Forest. In the Danube valley it is distributed where the rolled drift is found, but does not occur in the districts where tertiary rocks are met with. It thus avoids the highlands of Passau, but abounds on the Ems, where there is a wide development of diluvial drift.

It avoids the high mountain districts. It abounds in the Rhine valley, in that of the Maine as far as the districts marked by the Keuper beds, along the Neckar and notably near Stuttgart, but is entirely absent in the neighbourhood of Nuremberg. In the Riesengebirge it occurs to a height of over 400 metres, on the Erzgebirge to a height of 300, and in the Hartz to over 200 above the sea-level. It hardly occurs at all in the districts where primitive rocks prevail. It abounds in the valleys of the Maine and Neckar where the Muschelkalk is found, while it shrinks where the Keuper and Bunt sandstone prevail. It is hardly worth mentioning in the Maine valley about Bamberg, where the Keuper rocks are found, while its proportions swell again near Wurzburg, where the Muschelkalk prevails, shrinking again in the district of Buntsandstein of the Speisart. In the Jura it is very subordinate, as it is in the Silurian districts of Bohemia and in the Schiefer mountains of the Rhine, although it occurs in both districts near the great rivers. Such a distribution is quite incompatible with its having been deposited by ordinary diurnal methods.

It covers the higher terraces, but not the lower ones. In Germany the loess forms a continuous border to the drift with erratic boulders, and it in fact mantles over a zone of that drift, and shows therefore that it is closely united with it. It seems on every ground in fact to have been contemporaneously distributed with it, and, in my view, to be the deposit of the diluvial water when it had dropped its heavier burden at the earlier part of its journey.

Mr. J. Geikie says very truly that "the loess of Central

and Western Europe cannot be considered as a separate and independent formation. We find it again and again closely associated with river gravels and containing intercalations of clay, sand and stones. It is true that in the Rhine valley it retains a remarkably homogeneous character throughout a wide area and a great thickness; . . . but, as we have seen, in Belgium, and especially in the north of France, it loses much of its typical character; and this is still better exemplified in the valleys and low grounds of the south of England, where the loess beds are composed in large measure of brick-earth, in which sand and even gravel are frequently intercalated."

It is not only in its contents that the loess agrees with the diluvium and the brick-earths: the mode of their occurrence in places quite independently of the drainage; covering the plateaux as well as being found in the valleys, in some places accumulated to great depths; in fact, in every particular, save that of internal structure and the absence of carbonate of lime, they agree with the loess, and precisely the same arguments apply to it that we applied to them.

In regard to the loess of Russia Mr. Hume says: "It lies unconformably on all the principal formations. To the west of the Dnieper it hides beneath itself the broken and contorted gneisses and granites of the Archean axis; in South Ekaterinoslav and the Don Cossack country it covers the shales and sandstones of the carboniferous, while in the more central governments of Kursk, Kharkoff and Tchernigov it overlies the cretaceous and the whole tertiary series; also along a certain definite line running to the north of these governments it rests upon the boulder clays and sands of the glacial period." This shows that the loess is a true drift, and not due to the disintegration *in situ* of the subjacent beds. The subjacent beds no doubt alter its form in a slight degree. Thus it has been noticed that near the Archean rocks it is much richer in micaceous materials, but substantially it is uniform. The loess of Russia, again, agrees with that of Central Europe in its organic contents, and therefore in its date. "The mammoth remains, which are so common in the Don country, all hail from this deposit. Indeed, so common are such mammalian finds, that bones are constantly being brought

to light in all parts of the country. . . . The fauna of the Russian loess therefore resembles that mentioned by Braun as occurring in the Rhine valley and at Toulouse" (*Geol. Mag.*, 1892, p. 554).

As with the more recent explorers of the German loess, Mr. Hume closely connects the loess deposits of Russia with the so-called glacial beds, with which view I am in complete agreement. He says: "In North Russia there is nothing but glacial drift at the surface, while in the south the loess entirely takes its place, but between them is a band, passing into Galicia and Saxony, along which the loess overlies the glacial drift. . . . This relationship appears too striking to be casual" (*ibid.*, p. 560). Mr. Hume accordingly attributes the loess of Russia to a glacial origin. He cites Baron Richthofen, the author of the æolian theory, as conceding that the period of the loess deposition in Central Asia was coincident with the glacial period in Europe. This view of the glacial origin of the loess is also adopted by Prof. Dokoutchaieff in a note published in the *Bulletin of the Geological Society of Belgium* for January, 1893, on the loess of South Russia. *Inter alia*, he argues that the fine-grained loess of the Poltava or Nijni Novgorod type, of uniform structure throughout its thickness (except where a few small erratic blocks are present in its lower portions), is of exclusively glacial origin; and he argues that it represents the finer constituents of the glacial mud deposited over a country already covered with vegetation and inhabited by the typical rodents of the steppes. A similar loess occurs sporadically as little islands throughout the whole of the district, bearing traces, he argues, of glaciation, sometimes in the inferior sands containing erratic blocks, and sometimes in the northern brown clays. The northern loess apparently contains a larger proportion of grey loam, and it becomes more calcareous as we proceed southwards. On the littoral of the Sea of Azov marine are interstratified with glacial deposits (*i.e.*, with loess). In his summing up of the Russian professor's case, Mr. Hume says: "Seeing, then, that the typical loess is, in the government of Poltava, restricted to the region of inferior moraine deposits, and only extends beyond them to a slight extent; viewing the fact that it is completely analogous to glacial mud

occurring in purely morainic deposits, erratic blocks also being sometimes found at its base; bearing in mind also that loess may be found below the brown morainic clays, *the loess must be considered as a glacial mud deposited from glacial waters*". Mr. Hume adds "that along the boundary of the old Russo-Scandinavian glacier, a boundary indicated by moraine clays containing rocks of Finnish origin, the loess immediately overlies the boulder clay and is stratified at its base and clings closely to the old line of ice action" (*Geol. Mag.*, 1894, p. 306).

Translating this into my language it means that there is a complete continuity between the boulder drift of North Russia and the loess of South Russia, a continuity of date and a continuity of origin.

It is an easy step from the loess to the *tchernozem* or black earth. In regard to the "black earth" Mr. Hume says: "The first point of importance is the close connection existing between the loess and the black earth. Murchison included both deposits under the common name *tchernozem*. . . . Schmidt regards the black earth as practically identical with loess (*Zeits. Deutsch. Geol. Soc.*, xxix., pp. 830, 831). Richthofen says the black colour, which is proper to the uppermost layers only, appears to result solely from the formation of vegetable mould, the deeper portions showing the brown colour of the loess, together with its structure, although this appears to be less perfect than in the former case. "Personal observation," says Mr. Hume, "has left no shadow of a doubt on my mind as to the intimate connection between those two types of deposit. Reference has already been made as to the gradual passage from pure loess to black earth observed in a ravine at Kharkov, and an example of the interlamination of the two at their point of junction has been cited from Bielgorod. . . . Repeatedly the dark-brown humus-bearing beds form distinct lenticular patches (often three or four feet in thickness at the centre, and extending some little distance in a horizontal direction) running through the yellow clay. The gravel stage is reached when the black earth forms a distinctly marked stratum, actually overlain by true loess. This is beautifully observed in the ravine near the railway station at Kieff, where a thick layer of black earth is overlain by some seven feet of loess."

"But the inter-relationships of the two deposits are still further brought out if a closer examination be made of the materials composing them. Not only does black earth agree with loess in its superficial position, its existence at most varying level, its occasionally purely local development, but it possesses the same want of consistency when rubbed in the fingers. The chemical composition is in most respects similar. . . . Both in character, position, chemical analysis and history the black earth is thus intimately connected with the loess, and is due to special circumstances affecting the latter" (*Geol. Mag.*, 1894, p. 307, etc.).

Murchison, describing the black earth, says it lies on rocks of all ages, and is found at very different levels. The northern drift is succeeded, if not actually overlapped, by the black earth. He considered it as not having been formed *in situ*, but as being derived from the disintegration of the black jurassic shale which is found in the more northern districts of the empire. It is, in fact, a true drift.

"The *tchernozem* or black earth of Southern Russia occupies," says Mr. Geikie, "the same geological horizon as the loess, and its origin is undoubtedly closely bound up with that of the former. It extends over the steppes and low-lying plateaux that border on the Black Sea, the Sea of Azov, and the depressed area to the north of the Caspian, with a breadth from north to south of from 200 or 300 to nearly 700 miles. It may be said to continue with little interruption from the regions watered by the Pruth and the Dniester to the foot-hills of the Ural Mountains between Ufa and Orenburg, thus comprising an area of not less than 500,000 square miles. Throughout this wide tract the black earth shows a singularly uniform character. Like the loess of Central Europe, it has an extremely fine texture, and is usually devoid of well-marked stratification. It varies in colour from dark brown to black, and in thickness from a foot or two up to twenty, and it is said even occasionally to sixty feet. . . . This is a very fair description of the black earth, and shows its close connection in origin, texture and date with the loess with which it is continuous. In another direction the black earth is similarly shown to be continuous and closely connected with the drift beds of Northern Russia. Murchison and his associates

state that the materials of this drift, consisting of stones derived from the north, are reduced to small size and mixed with the débris of local rocks as they approach the northern margin of the black earth, by which deposit they are succeeded if not overlapped. At one place, however, they observed erratics or travelled stones of northern derivation superimposed on the black earth."

The loess and *tchernozem* of European Russia are not limited to that area. They extend far beyond it, and form a large part of the surface deposit of Central Siberia and of the Mongolian steppes, and reappear again developed on a great scale in Northern China, where loess occurs over a vast area, and presenting the same features as the European loess, the same concretions, the capillary tubes, the land and fresh-water shells and bones of extinct animals, the same tendency to cleave in straight facies, the same large element of calcareous matter in it, the same heterogeneousness, and the same signs and evidences of its having been laid down over hill and dale by some widespread force acting rapidly and independently of ordinary gravitation.

This completes our survey of the drift deposits of the Old World. We are not discussing their origin in this chapter, but their continuity. Of this it seems plain to me there can be no doubt, and that the glacialists are quite unjustified in drawing their arbitrary frontier lines. The continuity is complete between the boulder drift of the far north and the loam and loess of the central and south-eastern parts of Europe, and continuous also with the intermediate beds of sand and gravel. They all speak of widespread continental transport, and I fail in this view to find evidence of those boundary lines between the glacial and the non-glacial areas in the drift districts with which some famous works abound.

If we turn to America we have similar evidence of the existence of contemporaneous and intercalated beds of true drift reaching southwards far beyond the range of the supposed ice-sheet. These are in two forms—in the form of what the Americans call extra morainic drift or "fringe," and in the form of loess.

The main frontier of the supposed ice-sheet in America is what is called the great terminal moraine. The name fringe

was, I believe, first given to the deposits under discussion by Prof. G. F. Wright and Mr. Carvell Lewis, and includes the dispersed erratics and thinner so-called glacial deposits which extend in advance of the moraine. These writers tell us how in Pennsylvania transported boulders occur occasionally upon several hilltops in front of the moraine, and in New Jersey unmistakable evidence of this fringe had been noticed a few miles south of the well-defined moraine. Prof. Salisbury mentions similar phenomena in New Jersey, and says, *inter alia*, "In Pennsylvania there are drift deposits well south of the moraine consisting of glaciated boulders in clay several hundred feet above the sea-level".

It is curious to find Dr. Wright, who tries to summarise the effect of this evidence, joining the army of the faithless in regard to so-called glacial striæ, and saying of some scratches on stones at a place called High Bridge: "*The scratches might well have been made in the process of creeping down the disintegrating mountain side, which secures almost exactly the same mechanical force as the movement of a glacier does.* . . . In the valley of the Pohatcong at and below Washington the foreign material is so abundant and of such size as to make it probable the glacial ice over-rode Scott's Mountain. . . . These boulders increase in numbers on the north side, but are mingled in irregular fashion with the local débris. . . . It is important to notice that there is continuity in the distribution of this foreign material from the moraine southwards over Scott's Mountain into the valley of the Pohatcong at Washington." He speaks of Medusa and Oneida boulders as deeply covering the country, and extending as far as Musconetcong Mountain, within five miles of the Delaware, one of these boulders measuring 3 × 2½ × 1 feet, while another retained a very perfectly scratched surface.

At Bridgeport, which is about thirty miles south of the terminal moraine, where he had noticed such deposits, he says that glaciated boulderets were taken from clay of such character there, that if the locality had been known to be covered with ice they would certainly have been referred to till. Salisbury (*Rep. of State Geol.*, 1891, pp. 106, 107) attributes these phenomena to an older ice age, and he says that "in its southern extension the ice extended to the region of the

yellow gravel formation". He further says that at High Bridge and Pattenburg, boulder clay is found twenty miles south of the moraine, and speaks of a locality fifteen miles south-west of New Brunswick where similar phenomena occur. Wright further mentions that, both in Eastern Pennsylvania and in portions of New Jersey, the fringe extends a considerable distance beyond the moraine (*Proc. Acad. Nat. Sci. Phil.*, 1892, p. 478). He especially refers to the evidence of the Susquehanna terraces, of which he says: "The occurrence of granitic pebbles in these terraces is of great significance, since it fixes them as contemporaneous with, or subsequent to, the glacial period, for there is no outcrop of this material anywhere in the watershed of the Susquehanna above Harrisburg".

Dr. Wright rather labours the point of the presence or absence of foreign stones in this extra morainic drift. As we have seen, the essential point is not this, but whether the surface deposit is a true drift and distributed independently of the present contour of the country. His general conclusion is that on the Atlantic coast, as in the Mississippi valley, there is usually a fringe of thinner glacial deposits extending a few miles more or less south of any well-defined moraine, and, further, that the facts do not lend support to the theory of a discontinuity between the drift north of the moraine and south of it, with which moraine it has both a genetic connection and a moderately close time relation. "The drift material is the same with a larger addition of local gneissic rock, and it does not, as Prof. Salisbury argued, point to another and earlier movement than that which deposited the terminal moraine" (*ibid.*, pp. 473-484). "Prof. Salisbury in his report correlates this fringe deposit with the yellow gravels of the country to the south, over which, he claims, the ice-sheet also travelled, mixing with it more northern materials. In this event, he says, boulders would have been carried beyond the edge of the ice-sheet by floating ice, and scattered over a wide area beyond the reach of the glacier, so that a sharp line marking the limits of the drift or even of glacier drift would not exist" (*op. cit.*, p. 108).

Lewis refers to the so-called* terminal moraine in Pennsylvania, which, he says, Chamberlin and other geologists have described as the terminal moraine of the second glacial epoch.

Their reason for this opinion has been, he says, that there are certain deposits outside of the moraine which they believe to have been made by an ice-sheet of the first glacial period. This "first glacier," like the supposed one in Switzerland, has left no polished or striated rock surface, and no moraines or kames, to prove its existence. This drift outside the terminal moraine he calls the fringe, and says that in Western Pennsylvania it is less than five miles wide, *apparently unglaciated*, yet with great erratics brought from Canada and perched on the hills, and also with occasional thin deposits of clayey drift somewhat resembling clayey till. The hilltops bearing these boulders show no signs of glacial erosion, being in all respects identical with those south of the limit of all drift. "In Dakota, far outside of the moraine, there occur clusters of granite boulders upon the hilltops often unaccompanied by till or gravel, the boulders having been transported from the Laurentian region, nearly five hundred miles away. The rock on which they rest is not striated or disturbed, and the boulders finally cease with no sign of a moraine bounding them. . . . The supposition that this extra morainic drift was made by a former extension of the ice is not supported by what we know of the action of glaciers elsewhere. It is inconceivable that the laws of glacial action, as we know them in modern glaciers, should be violated by the first glacier, while followed perfectly by the second" (*The Glacial Geology of Great Britain and Ireland*, pp. 50, 51).

Let us now turn to the other great drift deposit of America outside the so-called terminal moraine, namely, the loess. In regard to the stratigraphical position of the loess there, there is a very interesting paper by Profs. Chamberlin and Salisbury in the *American Journal of Science*, from which I propose to quote freely.

They tell us that "between the parallels of 37° 30′, the southern limit of glacial drift, and latitude 35° the country is overspread by loess, which extends, however, much below this latitude. A similar belt of loess is disposed along the course of the Ohio for a considerable distance above its junction with the Mississippi, though the loess along this stream is less constant in character, and its facies often differ from the Mississippi River loess. As usual, the deposit is more developed

on the higher ground, where it generally keeps its characteristic loose, open texture. As we trace it eastward or westward from the Mississippi, or northward or southward from the Ohio, these characteristics gradually disappear. The open texture becomes less pronounced, and by almost imperceptible degrees passes from that of a loose light loam through that of a clayey loam to that of a loamy clay. So far does this gradation proceed that the texture of the loam at some distance from the streams resembles much more closely that of the residuary earth upon which it rests than that of the typical loess on the immediate borders of the valley.

"A change in colour accompanies the change in texture. The buffish colour which everywhere characterises the river bluff-loess in its normal development becomes deeper with increasing distance from the streams, so that along the borders of the loess, where the texture has come to simulate closely that of the residuary earths, the colour has become notably deeper than on the bluffs immediately fronting the river, and the deepening colour has been a constant approximation to the colour of the underlying residuary earth. As the texture of the loess becomes closer and its colour deeper with increasing distance from the rivers, the proportion of carbonates diminishes, and may entirely disappear before the border of the loess is reached. The shells and concretions are limited to the calcareous portions of the loess. With increasing distance from the streams goes another change in the composition of the loess. The complex silicates (feldspar, mica, hornblende, augite, etc.), which are found to be very significant ingredients of the formation along the river bluff, become less and less abundant as the other normal characteristics disappear. They may be found, however, in the loams far away from the rivers after almost every other loess feature has disappeared. Along the Mississippi, and along its tributaries in Illinois, Missouri, Iowa, Wisconsin and Minnesota, and along the tributaries of the lower Iowa, in Illinois and Indiana, the loess lies upon the glacial drift. In southern Illinois, for example, the drift for many miles north of its southern boundary is overspread with loess or with clay-like loam, which may be traced into direct continuity with the normal open-textured calcareous shell and

concretion-bearing loess along the immediate valleys of the streams. Here, too, in scores and hundreds of places, especially in south-eastern Illinois, it may be seen that the surface of the drift upon which the loess rests is one which gives no evidence of exposure to the atmosphere before the mantling loess was spread upon it. Had such exposure found place the fact would have left its record in the oxidation of the exposed surface, or in the accumulation upon it of an old soil, traces of which would still be found beneath the loess. But in south-eastern Illinois and the adjacent parts of Indiana no zone of oxidation and no vegetable layer or trace of old soil separates the loess from the till beneath. It would not be necessary to suppose that such a zone as that here referred to would necessarily be preserved at all points until the present time. But its universal absence over large areas under conditions which must have been favourable for its preservation, had it ever been developed, seems to be conclusive against the hypothesis that it ever existed. That the conditions were favourable for its preservation is proved by *its well-nigh universal presence under the loess immediately south of the drift border in the same region.*

"In many places it may be clearly seen that the superficial loess mantle and the stony drift beneath meet each other in a thin zone of gradation—that is, the pebbles of the drift frequently occur in the basal portion of the mantling loess in and just above the horizon, where the imbedding matrix changes from a gritty clay (till) to a gritless loam or loamy clay (loess). In other places there is a more or less marked accumulation of drift pebbles immediately below the loess or its clayey equivalent, marking its junction with the till. In the judgment of the writers, therefore, the relationship between these two deposits, the till and the loess, as seen in innumerable sections in south-east Illinois and south-west Indiana, is such as to admit of no second interpretation as to their sequence. The loess in the regions where such sections are found was deposited immediately after the till, so far as not actually contemporaneous with it. We distinguish other sheets of loess contemporaneous with other stages of glaciation."

The two authors then go on to say that the material of the

loess is in part derived from the till beneath and in part from the glacial silt carried southward from the melting ice to the north. They correlate the loess with what they deem to be the deposits of the glacial epoch, making it the equivalent of the later part of that deposit. The continuity is complete, and the character of the formation is the same on both sides of the line which marks the limit of ice advance. It is a continuous mantle, overspreading alike the drift border on the north and the residuary earths which the ice did not disturb on the south. If, therefore, the age of the loess which covers the drift be first-glacial (first episode), the age of that which lies south of the drift in the area under discussion is likewise first-glacial.

"Between the relationship of the loess to the till north of the limit of glaciation, and the relationship of the loess to the residuary earths of the palæozoic rocks immediately outside the drift, there is one important difference. The presence of a weathered and highly oxidised zone immediately subjacent to the loess south of the drift limit is as conspicuous as its absence to the north. This oxidised zone is the upper surface of the residuary earths. There is in places a slight admixture of residuary material and loess at the junction of the two. But the body of the residuary earth is clearly separated from the body of the loess. The fact of the existence of a long interval between the loess and the residuary earths beneath is as clearly indicated on the one hand as is the fact of the absence of an interval between the loess and the underlying till on the other" (*American Journal of Science*, 3rd series, xli., p. 359, etc.).

In the *Popular Science Monthly* for July, 1893, Major Powell, who was then at the head of the United States Geological Survey, says "the loess is a formation contemporaneous with the glacial formation of the north".

In a paper on the loess and associated deposits of Des Moines, by McGee and R. Elsworth Call, published in the *American Journal of Science*, xxiv., 1882, we are told that in that part of Iowa the drift is everywhere superficially modified to a certain extent, the upper portion being fine, homogeneous, free from boulders and pebbles, though sometimes containing calcareous nodules, and loess-like in structure,

aspect and topographical configuration, but graduating imperceptibly into unmodified glacial drift within a few feet below the surface. Describing two plateau-like hills, one called Capitol Hill, near Des Moines, the authors say they are marked "by characteristic loess topography, but with numerous erratic boulders scattered over their summits". Drift, with many erratic boulders, is described as covering the entire summit of Capitol Hill. "The proportion of pebbles in this drift, however, is less than is usual in this latitude. The clay presents a somewhat loess-like aspect, contains calcareous concretions and yields a calcareous efflorescence, and the topography assumed is essentially identical with that of loess areas." In the *Proceedings of the American Association for the Promotion of Science* for 1878, vol. xxvii., McGee says loess often grades insensibly into the drift.

The object and purport of the bringing together of the foregoing perhaps too numerous facts and arguments is to press home the conclusion that in the northern hemisphere the so-called glacial deposits have no sharp and definite boundary along their southern frontier such as we find pictured and defined in the text-books which are now fashionable (*e.g.*, in Mr. Geikie's well-known maps). The beds of boulder clay, with their associated gravels and sands, as marked by northern erratics, and occupying so many degrees of latitude and longitude in the northern hemisphere, instead of coming suddenly to an end, as shown on these maps, gradually change in texture and contents. The number of northern crystalline stones gets smaller and smaller. In North Germany, where these beds are so markedly sandy, they gradually merge into the so-called Zand diluvium of Holland, and this, again, into the Campinian sands of Belgium and Flanders.

This sand, again, passes laterally into beds of brick-earth and loam containing angular fragments of flint, etc., and spread like them over the country independently of its contour, which are known to the French as *limon rouge*, *limon des plateaux*, *limon de Hesbaye*, etc. These loamy beds, again, in France pass laterally into loess of different kinds, which, like the true boulder clay, is unstratified, and like it lies directly upon the gravels which contain the remains of the mammoth and paleolithic man, and is quite unconnected in the great mass

of its materials with the beds underlying it. In Britain the continuity seems to me to be equally complete, and the notion of a sharp frontier in the Thames valley and a line prolonging it westward seems quite unsupported by the facts. In the west of southern England crystalline erratics occur, as we have seen, to the south as well as to the north of this line, while all over the area great masses of travelled Sarsens or greywethers occupy the place of the primitive erratics of the north country.

The so-called glacial gravels of Hertfordshire and Essex pass into the brick-earths and loams and plateau gravels of the southern counties, and especially into those beds known as "angular drift". The angular drift to the south of the English Channel is marked by the same Sarsen stones and the same distribution independent of the contour of the country, and merges into the *diluvium rouge* and loams of Central and North Central France. These contain the same kind of land and fresh-water shells and other débris which the corresponding beds do in England, and the Sarsens, the débris of the broken-up eocene beds, lie buried in them in the Somme valley, etc., in the Forest of Fontainebleau and in the Morvan.

As in England they are covered with "race" and loamy loess, so in Eastern France they are similarly connected with the true loess.

This, as we have seen, is the case not merely in the Old World. In America the same continuity exists. The tough boulder clays of the north, with their closely connected sands, pass into the fringes and "extra morainic drift," and these into the yellow gravels, and eventually into the loess.

The materials change, the texture changes, but the one supreme feature of a vast (to a large extent) unstratified mantle wrapping the country, irrespective of hill and valley, pervades the whole; and if men were free from *a priori* prejudice in discussing it, they would say that from the North Cape to the Carpathians and beyond, from Cape Wrath to the south of France, and from Labrador to the Gulf of Mexico, these mantles of widespread clay, sand, gravel and loam, with their burden of erratics, organic and inorganic, were due to some common cause. Let us now take another step in our journey.

CHAPTER XIII.

DERIVATIVE CHARACTER OF THE INORGANIC CONTENTS OF THE DRIFT.

"There is no field of geological observation where there is more avidity in drawing hasty inferences and forming generalisations from imperfect data than in that section which concerns itself with the occurrence, dynamical effects and hypotheses of causation in respect of former glacial action."—Johnston, *Glacier Epoch of Australasia*, p. 19.

In Chapter XI. of this work I argued that all the débris of animals and plants which have been treated as dating from so-called glacial times, and have given rise, *inter alia*, to the notion of mild interglacial climates, are in fact pre-glacial in the sense that phrase is used by the champions of the glacial nightmare, and further, that if we are in search of a biological basis for the theory of an ice age such evidence is nowhere forthcoming. All that the biological evidence goes to show is that at a certain critical juncture in the earth's history there was a great destruction of life and the scattering of its débris, and that over this scattered débris were laid beds of drift, which also incorporated into themselves as boulders a portion of the débris thus scattered. The remains of plants and animals found associated with the drift beds are in fact derivative and not *in situ*, and they evidence an older horizon and a time before the drift was distributed.

If this was the case with the organic contents of the drift beds, what about the inorganic ones—the sands, clays, gravels and boulders of the drift? Were these the product of the grinding down of solid rocks by the portentous mill so often invoked, namely, the ice age? or were they also derived from sands, clays, gravel and boulder beds already existing?

Prima facie it might be expected that if the former were the case the process must have taken a long time to carry through, and that in such a case we ought to have abundant

traces of the life of the period to point to as evidence of it, for in the world as we know it no conditions of mere cold are sufficiently severe to exclude the presence of living forms somewhere, and if they were excluded from the areas where the cold was most intense they ought to be forthcoming in other areas. There is, therefore, a strong *a priori* probability that the inorganic contents of the drift beds like the organic ones are derivative, and that they do not index a long period of time. These inorganic contents may be examined in two aspects—either in regard to their texture and form or in regard to their distribution.

No one can have read the literature dealing with the surface beds of these islands which has been published during the last two decades without being assured that a great change has come over the opinions of those who have discussed them. At one time the champions of the glacial period hardly gave a thought to the first origin of the inorganic contents of the drift. They seemed to take it for granted that the sands and clays and loams, the gravels and boulders, were the result of the grinding of the ice in the ice age, and were in fact some of the best proofs of its handiwork.

More recently another view has been spreading, namely, that most, if not all, of these drift beds are not the products of ice erosion, but are the *remanié* and redistributed contents of similar beds which were on the ground before, and this view will I take it prevail everywhere presently. The inorganic contents of the drift beds are in fact just as much derivative as the organic ones. The first and most obvious argument that may be used in this behalf is that the contents of many of these beds are apparently not far travelled, but are in texture and form like the beds underlying them, with the single important difference that they have been rearranged and redeposited. If this be so, then there is little or no necessity in a work on the so-called ice age for speculation about the method in which they have been ground down from a more solid matrix, for that problem is one connected with an earlier age. It is plain that at the time when the loose drift beds were laid down there were no available exposed solid strata similar to them in composition in much of the area to which we are re-

ferring from which they could have been ground down by any process, since all the solid beds were buried deep down below beds largely composed of soft materials; so that if the drifts are of local origin they must have been ready-made when they were redistributed. Let us examine them somewhat more in detail, and first in regard to the sands. The texture and microscopic appearance of the so-called glacial sands is strongly in favour of their being old sands which have been used up again and become quite weather-beaten and quite different to glacier-made sand. Thus, to quote an example or two, in regard to the drift of the Irish Sea basin, Mr. Mellard Reade says that the sand when separated from its clayey matrix is much rounded, some of the grains being extremely polished (*Geol. Mag.*, 1892, p. 312). The same writer says again: "The grains of sand in the sands and gravels of Tryfaen are much rounded, water-worn and polished. The till also contains a proportion of these highly polished grains, and on washing some of the locally formed till lying at a level of 800 feet above the sea between The Rivals and Mynydd Carngnwch I found that this also contained highly worn and polished sand grains" (*ibid.*, p. 313). "Mechanical analyses of twenty samples of till from the coastal plain near Tryfaen yielded the rounded and polished quartz grains." Reade, who has paid great attention to these rounded and polished sand grains, again says elsewhere "they distinctly proclaim their marine origin" (*ibid.*, p. 314). He also speaks of the sand associated with the boulder clay at the Seacombe railway cutting as consisting very largely of highly polished and rounded sand grains, and much worn, whereas the grains of the Keuper sandstones at Poulton are angular and those of the Bunter sub-rounded (*Proc. Liv. Geol. Soc.*, 1894-95, p. 343). Lastly, he describes many of the sand grains, as also the fine gravel in the lower drifts of Lancashire and Cheshire, as highly polished and rounded (*Geol. Mag.*, 1896, p. 489).

Mr. F. W. Martin also speaks of the sand grains in the drift at Birmingham as mostly rounded and cemented together; and of those in the pockets of sand he says they are much larger than those in the red sandstone rock, and mostly rounded and smoothed, and must have been transported from a distance (*Phil. Soc. Birm.*, v., p. 368).

Apart from the polished, worn character of the grains of which they are composed, the so-called glacial sands clearly proclaim their derivative character in other ways.

In East Anglia the drift beds in many places consist of sands which in texture, colour and other respects exactly resemble the red and yellow sands of the crag beds. There is no means of exactly separating them. They run into each other, and in mapping them the geological surveyors have confessed their inability to discriminate them. The discrimination has in most cases been quite arbitrary. When they are *in situ* and apparently unmoved they are called crag sands, or barren sands of the crag. Where they have been shifted and moved they are called middle sands or glacial sands. It seems as plain as plain can be that in this area the so-called glacial sands are simply redistributed crag, and that they are truly derivative. Let us turn to some concrete cases.

Originally the middle sands were classed with the crag, and notably with the red crag, over the region of which they reach their greatest thickness, and were termed "the unproductive sands of the crag". It was, I believe, Mr. Searles Wood who first definitely separated them; but even he says that it is not impossible that the lower part of them, which is sometimes very red, may belong to the pebbly sand (*i.e.*, to the so-called Westleton series), the whole sand mass presenting no line of division and being unfossiliferous alike (*Introduction to the Crag Mollusca*, p. xxi).

The reasons Wood gives for separating the so-called middle sands from the crag seem very insufficient, and depend on the presence of the Chillesford beds between the typical crag and the so-called glacial sands in certain places; but these latter are very locally distributed, and seem to me to distinctly belong to the crag formation, and are now generally so classed. In regard to the evidence of the shells, this has been answered by Mr. Horace H. Woodward, and I have quoted his views on the subject in an earlier page.

It must be remarked as very strong evidence of my contention that so great is the difficulty in some cases of distinguishing between the true crag and the so-called middle sands in this area that in many cases the geological surveyors have

left the question doubtful. The texture and colour of the sandy and gravelly beds and of the loamy seams is the same as is the fact that in some cases the beds are full of shells and in others quite barren, while, as we have seen, the marine shells are the same in both, or, as Mr. Wood puts it, there is not a single shell in the East Anglian drifts which is not derived.

In the memoirs of the Geological Survey for East Anglia there are continual laments about the difficulty of separating the so-called middle sands or glacial sands from the sand beds of the Norwich crag. Thus, to quote some examples, Mr. H. Woodward says, in his *Memoir on the Country round Norwich*, "In many places there is a considerable difficulty in drawing a definite line between the glacial sands and the Norwich crag when they do not contain foreign stones" (*op. cit.*, p. 104). Mr. Blake speaks of the middle sands at one place south of Gorleston as shelly for a thickness of nine feet and as having in places the appearance of crag, the fragments being very numerous (*Geology of Yarmouth and Lowestoft*, p. 39).

Speaking of the red crag 600 yards south-west of Melton Church in Suffolk, Mr. Dalton says before the principles of decalcification and oxidation were recognised, it was figured by Messrs. Wood and Harmer as an example of a mass of crag undermined and partially surrounded by glacial beds (*Geology of Alborough*, etc., p. 15).

Two beds of yellow sand without gravel and with some comminuted crag in it, found at a depth of eighty to eighty-three feet at Woodbridge, were regarded by Mr. Wood as of lower glacial age, the crag being *remanié* (*ibid.*, p. 55).

Speaking of certain sands in a section between Carlton and Rendham, Mr. Dalton says the classification of the sands is somewhat doubtful. "Possibly both may be drift, but, on the other hand, both may be crag" (*ibid.*, p. 57).

Whitaker in his memoir on the Southwold district treats a large portion of the unfossiliferous sands as upper crag; but he speaks of its being almost impossible to draw a line of division in the midst of a mass of sand that is of much the same character throughout. He treats the pebbly gravel as the boundary between the crag and the glacial sands (*op. cit.*, p. 6).

The explanation of the difficulty of separating the two sets of beds seems simple enough. There is no difference except that of arrangement between them. The same sands when they occur as horizontal or undisturbed beds are treated as tertiary sands; when they have become contorted and dislocated or mixed with foreign stones they are classed as glacial sands; the fact being that the latter are nothing more than tertiary sand *remanié*, *i.e.*, are derivative. The so-called contorted drift is probably nothing more than a series of alternating crag sands and laminated clays, which have been twisted and contorted by some external force which has also introduced a series of foreign boulders into them. This mixing and tossing about constitute the sole testimony they offer to their being other than true and normal beds of crag.

While a large part of the so-called glacial sands or middle sands of East Anglia are derived from the crag, the rest are probably derived from the still older sand beds of the Bagshot and Reading series, those tertiary beds having doubtless before the denudation of the Fens occupied a much larger area in eastern England than they do now.

In a large number of cases where these sands have been mapped as glacial sands by the geological surveyors, I believe it has been through a mistaken notion that crag sands, or Bagshot sands, must necessarily contain crag fossils, or Bagshot fossils, whereas a large portion of the tertiary sands are barren and are only fossiliferous in places.

My own reading of the evidence is that the so-called middle sands of East Anglia are nothing more nor less than rearranged and *remanié* crag deposits; that they mark no geological period or horizon at all, but simply a transitory convulsion during which the previously regularly assorted crag beds had in some cases their upper layers, and in others the whole mass, rearranged and redistributed, and that they have as much to do with any glacial conditions as the gravel sand and mud deposited by the Holme Firth flood had.

What is true of the so-called middle sands of East Anglia seems to be equally true of the similar deposits in north-west and north-east England. The recognition of the shells in

these beds as derivative puts them on the same level as the similarly derived shells in Norfolk and Suffolk, namely, on the horizon of the latest crag deposits. It has always seemed curious that we should have found no traces of the Norwich crag in western England; but this discovery supplies the gap, and we may take it that the sands in which the derived shells occur are themselves derivative sands and belong to the crag series, just as the similar sands do in East Anglia.

In regard to the so-called middle sands of Lancashire and Cheshire, my friend, Dr. Ricketts, speaks of them as they occur at Bootle, as forming a light green, sandy matrix, which is very conspicuous from its colour being so different to that of the boulder clay. He adds that its deposition must have occurred in the same manner as the patches of loose sand frequently seen in the boulder clay which have been derived from the disintegration of triassic rock, and forming the base on which the boulder clay there rests (*Proc. Liv. Geol. Soc.*, 1878-79, p. 8).

"On examining the sands from the glacial deposits in the Isle of Man and generally throughout the east and south shores of the Irish Sea we find that they are mostly of triassic origin" (Herdman and Lowson, *Floor Deposits of the Irish Sea*, p. 213).

The discovery by Jamieson of true crag beds *in situ* in Aberdeenshire explains for us the origin of the sandy beds which occur with the till in north-eastern Scotland, and point to the matrix of those beds as well as their shelly contents being derivative. It is not only the sands which seem to be thus derivative, for Jamieson says: "If I am right in referring this sand and gravel to the age of the crag, it will throw some light upon the origin of our chalk flints, for as flints are common in these beds, it would show that they could not have been drifted hither during the glacial period, but must have existed in the district before" (*Quart. Journ. Geol. Soc.*, 1860, p. 372).

If we cross the North Sea we can hardly doubt that those geologists are right who have considered a large part of the Geest and the Campinian sands of Holland and Belgium as derived from the crag beds which are so largely and widely distributed in the Netherlands.

In Holland, from Brabant to the Helder, the surface beds, which are classed by some as glacial, lie immediately on crag beds which are there developed on a great scale, while there are no accessible beds of secondary clay or chalk exposed which could supply the ingredients for making boulder clays like our chalky clay, etc. These so-called glacial beds in Holland consist, therefore, merely of sands and pebbly gravels corresponding to the middle sands which we have been discussing, and consist, as the similar beds do here, of rearranged crag beds, with a certain mixture of erratic boulders, and nothing more. The sandy matrix of the beds is of home growth, and simply testifies to the crag beds having been taken up by some mighty engine which has rearranged and tossed them about.

M. Elie de Beaumont argues that the Campinian sands are to all appearance derived from the degradation of the sands of the older tertiaries, upon which they rest, both at Maestricht and in part of the Campine, while included in them are an abundance of flints mixed with erratics from the Ardennes and the mountains skirting the Rhine.

Sir Charles Lyell points out the difficulty of distinguishing between the sandy base of the loess and the eocene sands upon which it rests, by reason of the occurrence of a large number of derivative fossils in the former washed out of the latter. Lyell also instances cases in which M. Dupont, from this presence of derivative fossils, regarded as of loess (Campinian) age beds which he was inclined to refer to the eocene. Searles Wood says that as the Campinian beds thin out towards their margin near the Ardennes their distinction from the subjacent eocene becomes probably more obscure than it is further to the north, where, from their greater thickness and from the presence of their included erratics, a general concurrence of opinion exists as to their occurrence beneath the loess (*Ann. Mag. Nat. Hist.*, 1864, pp. 9, 10).

D'Archiac says that in the "Campine" as in Poland it is probable the sand has not come very far; and he argues that it is really the rearranged and resorted tertiary sand answering to our upper crag (*Hist. des Progrès*, etc., ii., pp. 142, 143).

The same writer, in his *Histoire des Progrès*, etc. (ii., pp.

142, 143), speaks of the drift sands of Westphalia and of Poland as rearranged and resorted tertiary sand answering to the English upper crag.

In regard to the sand which forms so much of the drift of North Germany, the local geologists are very unanimous that it is in a large measure, if not entirely, the rearranged and redistributed sand of the so-called brown-coal formation which constitutes so much of the subjacent strata underneath the drift beds in that area.

Thus, as far back as 1821, Schultz (in his *Beitr. zu Geog.*, and also in his *Grund und Aufrissen zur Bergbaukunde*, 1823) urges that the sand under the blocks in Germany belongs largely to the brown-coal formation, and not, as Sefstrom urged, to the diluvial time.

In America, again, it would seem that the so-called glacial sands are in great part rearranged beds of the orange sands and sands of the Lafayette beds, which answer to our crag.

Prof. G. Dawson describes the drift material in the prairie region of Lake Winnipeg of North America as consisting in great part of local débris derived from the immediately underlying soft formations (*Quart. Journ. Geol. Soc.*, 1875, p. 611).

Let us now turn to the boulder clays. A very new and startling difficulty for the extreme glacialists has arisen in the last few years in regard to these clays. They had very pertinaciously, and in spite of all kinds of arguments to the contrary, been explained as the result of the grinding of great ice-sheets against their beds, as in fact the products of land ice, and when it was pointed out that they were full of shell fragments, these were treated as boulders. But we cannot treat as boulders the *foraminifera* and *ostracoda*, which are so minute in size and so intimately mixed up with the clay matrix, and which have been shown to exist in such large quantities in apparently all the boulder clays in the maritime districts of both Europe and America. Nothing seems plainer from the evidence of these organisms than that the clays containing them must be a submarine deposit and not a deposit from land ice, and, secondly, that the clays were not made *in situ*, but are themselves derived from some submarine beds of clay in which the protozoans lived.

If such an ice-sheet actually made these clays by grinding them down from Silurian slates, etc., how came the foraminiferal shells to be there at all, for they belong to quite a recent horizon? And if they did not do so, but actually belonged to the primitive beds whence the clays are supposed to have been derived, which is contrary to the view of all the experts who have examined them, how is it that the *foraminifera* were not ground down into similar clay? If, on the other hand, the clays were ready-made and contained the *foraminifera* distributed through them as we find them, then these clays are derivative, which is the conclusion for which I am arguing.

In regard to the presence of these *foraminifera* in the clays I will now quote a few passages.

The first person, I believe, to call special attention to their existence in the so-called glacial clays was Mr. Jack, who described them as occurring in the till of Loch Lomond as far back as 1874 (*Trans. Glas. Geol. Soc.*, v., pt. i., p. 5). Since then they have been found in many districts, and in fact they seem to occur in all the so-called glacial clays in the maritime districts everywhere.

In the *Quarterly Journal of the Geological Society*, 1874, pp. 181-185, Mr. Shone describes the discovery of *foraminifera* in the boulder clays of Cheshire (see also *Midland Naturalist*, 1878, p. 292). These *foraminifera* were found in the interior of the shells of *Turritellæ* and not in the clay matrix.

Among those who have most assiduously examined and reported upon their presence in the clays none has worked harder than Mr. Joseph Wright. He tells us that in regard to the north-east of Ireland he found *foraminifera* in all the clays in which débris of shells had occurred, while he had also found them in places where no shells occur, the specimens being usually in the finest preservation, as if just brought up by the dredge from a recent sea bottom. "Nearly all the *foraminifera* so found," he says, "are now to be met with living almost everywhere around our coast at moderate depths in quiet water and where the water is muddy, and we may presume that the *microzoa* of the boulder clay lived under somewhat similar conditions." Generally speaking, he says, the *foraminifera* are smaller than living specimens, while

he adds that they are all of marine types, and that he did not remember ever finding a brackish water *foraminifer* in boulder clay.

At Kill o' the Grange, on the southern shore of Dublin Bay, a thick deposit of boulder clay, capped by gravels, was explored by Prof. Sollas and Mr. Praeger. Specimens of it were sent to Mr. Joseph Wright, who described it as abounding in *foraminifera*, the species being such as occur in shallow water round our coasts (see lists, *Irish Naturalist*, 1895, pp. 324, 325). Some of the *foraminifera*, which could be readily distinguished by their chalky texture, were apparently of secondary age, and were probably derived from liassic and cretaceous beds.

Mr. Wright says the *foraminifera* from Ballyrudder, on the coast of Antrim, are larger than the normal size, and he gives measurements of them (*ibid.*, p. 344, note). He also refers to the boulder clays at Ballyhornan Bay, County Down, and Colligan Bridge, in the same county, as remarkable for the abundance of the *foraminifer*, *Noriconina depressula*, found there (*ibid.*, p. 344).

Mr. Joseph Wright has further reported on samples of clay from what Mr. Mellard Reade calls the upper and lower boulder clay and the associated sands near Liverpool. In the sands he found no *foraminifera*. This is usually the case with the sand. In the upper clay they were rare, but in the lower clay they were very abundant, and he publishes a list of them (see *Proc. Liv. Geol. Soc.*, 1894-95, p. 335). He found as many as 450 specimens in the sample from the lower clay, of which 380 belonged to one species, *viz.*, *Noriconina depressula*.

Mr. Wright also reported on samples of clay from the Seacombe railway cutting. He found *foraminifera* very abundant (see lists, *ibid.*, pp. 343, 344), and says they are such as might be found in any shallow water or muddy water around our coasts. As is usual, these *foraminifera* from the clays are, however, less in size than those now living.

In a paper on the clay at Great Crosby, Mr. Wright gives lists of *foraminifera* from two samples. He observes generally about them: "It is interesting that here, as well as at Poulton, Cheshire, as also at Ballyhornan Bay, County Down, and Colligan Bridge, County Down, *Noriconina*

depressula occurs in such great profusion". Again he says: "To give an idea of the profusion of the *foraminifera* in the clay examined, I may point out that they occur at the rate of over half a million individuals to the ton". In the same paper is a list of *foraminifera* from samples of the lower boulder clay from the Blackpool cliffs (*Proc. Liv. Geol. Soc.*, 1895-96, pp. 387-390).

In 1897 Mr. Mellard Reade described the contents of certain clays from Llandulas, etc., in the Vale of Clwyd, in which Mr. Wright found a large number of *foraminifera*. In his concluding remarks Mr. Reade says: "It is interesting to observe from the lists and localities given that the marine boulder clays of Lancashire, Cheshire and Denbighshire contain more frequently, and in a much greater profusion than was suspected, the tests of *foraminifera*. As a rule they are found in the finer or more plastic brown or red boulder clays, which often contain intensely striated erratic stones, and they occur in just the sort of deposits in which they are mostly found at the present day."

Speaking of the shelly patches at Bridlington, Lamplugh says "most of the clay patches show a curious mixture of dark green sand containing *foraminifera*".

The *ostracoda* and *foraminifera* from these patches were described by Dr. Crosskey. In regard to the *ostracoda*, he says all the species except two are found in the so-called glacial clays of the east of Scotland, where they occur with arctic shells. Of the eighteen species of *foraminifera* described from the Bridlington beds by Prof. Jones and others in their memoirs, all save one have been found living in the North Atlantic, and fourteen in the arctic seas. Nearly all of them occur in the Scotch so-called glacial beds (*Quart. Journ. Geol. Soc.*, 1884, pp. 315-327).

Dr. David Robertson, in his report on the so-called glacial clays from Kintyre, gives a long list of the *foraminifera* they contain (see *Brit. Assoc. Rep.*, 1896). He also described the tests of the same organisms from the shell beds at Clava on the north-east coast of Scotland.

In addition to the clays of the north-east of Ireland, Mr. Wright also reports on those of the south-west of Scotland in the neighbourhood of Glasgow.

There, he says, the organisms are as perfect and fresh looking as those found in the boulder clay of Ireland, a young specimen of *Pelystomella crispa* from Hamilton Hill being ornamented with long delicate spines in the finest preservation. Some of the Scotch specimens were larger than the Irish ones, which are all very small. In concluding his paper Mr. Wright says that, contrary to the view of most English and Scotch geologists, he was forced to the conclusion that the Scottish as well as the Irish boulder clay is a true marine sedimentary deposit (*Trans. Glas. Geol. Soc.*, 1894, pp. 263-272). In a paper by Mr. James Neilson (*ibid.*, pp. 274-279), also on the boulder clays of the Glasgow district, he claims to show, to use his own words, that "these remains were not 'boulders' (if it be permissible to call a *foraminifer* a boulder) at all, and that the land ice theory of the origin of boulder clay must be given up," and he describes a number of localities near Glasgow whence he took the samples of clay which he forwarded to Mr. Wright for examination.

In replying to some criticism, Mr. Reade said: "The late Dr. Robertson examined boulder clay from the Atlantic Docks, Liverpool, for the author, and the list of *foraminifera* is given in his paper on the drift beds of the north-west of England. It was not, however, until the boulder clays in the Wirral railway cutting, Cheshire, were examined by Mr. Wright and found to be rich in *foraminifera* that the author realised the importance of the subject. Since then many samples of clays from Great Crosby, Lancashire, Blackpool, the Vale of Clwyd and Ayrshire have been examined by Mr. Wright with the same result, . . . the facies being very similar from all the localities in England, Wales and Scotland" (*Quart. Journ. Geol. Soc.*, 1897, pp. 342-348).

The boulder clays of the Baltic lands are also in many places charged with *foraminifera* and *ostracoda.* Some of these are supposed to be derived from secondary rocks, chiefly cretaceous, but this is only a certain portion, the rest being clearly of pleistocene age, as the similar microzoan remains are in Britain and America. They have occurred in various places in Denmark, Holstein, the Danish islands, Sweden, etc. In Holstein and Denmark Madsen has made a special study of the foraminiferal fauna, and found it to be

much more widely distributed in the clay beds than had been supposed. A rich deposit has been described from Resbierge Klint in the island of Langeland by F. Andersson. At Dombusch in Rugen *ostracoda* and *foraminifera* have occurred in the so-called *Cythera-thon* and are referred to by Madsen and H. Munthe, the latter of whom has described them at great length in his memoir, *Ueber ältere Quartärablagerungen in Südbaltischen Gebiete*, pp. 40-50. A similar find is reported by R. Credner from the east coast of Jasmund in Rugen (*ibid.*, p. 51). Munthe describes a large number of *foraminifera* from the boulder clay of the island of Alsen in Schleswig (*ibid.*, p. 60, etc.), from Rögle Klint in Fünen (*ibid.*, pp. 85, 86), Tarbek in Holstein (*ibid.*, pp. 88, 89), and Burg in Ditmarsh (*ibid.*, p. 94).

In his *Canadian Ice Age* Sir William Dawson gave an annotated list of the *foraminifera* found in the Leda and other clays of Canada at Montreal, Quebec, Murray Bay, Anticosti, Rivière du Loup, Portland (Maine), Labrador, and British Columbia, and he says that most of the species still inhabit the gulf and river St. Lawrence, and are indicative of depths not exceeding 100 fathoms. The *foraminifera*, he says, are very generally diffused in the pleistocene clays, and are usually in as fine preservation as recent specimens, especially in the deeper and more tenacious layers of the Leda clay (*op. cit.*, pp. 211-216).

In a paper headed "Are the boulder clays of the great plains marine?" the late Dr. G. Dawson gives the result of the examination of some samples of American boulder clays by Mr. J. Wright. These comprised four specimens from the interior of the country; two from the Saskatchewan River, twelve miles below Victoria, 1,850 feet above the sea level; another from near Victoria, from 1,900 feet above the sea; a fourth from Selkirk, Red River, Manitoba; and a fifth from Ottawa, in Canada. Mr. Wright reported that he had found *foraminifera* and *radiolaria* in the specimens from the Saskatchewan, of which he says, "I am of opinion they are contemporaneous with the clay, and not derived from cretaceous strata. All the species were referable to recent species, and, with the exception of *Cristellaria Italica* and *Rotalia orbicularis*, have been found in the boulder clay. *Nonionina depressula* is the

most abundant form in our boulder clay, and it is instructive to find the species so common with us also occurring in your (*i.e.*, in the American) clay. *Bolivina lævigata, Cristellaria Italica*, as also some of the other specimens, have the clear hyaline lustre of recent specimens." "If cretaceous (as had been suggested), we should expect," he says, "to find *Globigerina cretacea* and *Textularia globulosa* plentiful." The cretaceous *foraminifera* which he had examined from America were filled, says Mr. Wright, with calcite, differing in that respect from most of those of the same age in Great Britain, but none the less stony and unlikely to float during the treatment of the clays. In Yorkshire he had met with clays containing about equal proportions of cretaceous and pleistocene *foraminifera*, but found no great difficulty in separating the two lots by the criteria already alluded to (Dawson, *op. cit.*, p. 258, etc.).

Turning from the evidence of the *foraminifera* they contain, to the nature of the clays themselves, those who have studied them in East Anglia are generally agreed that they are very largely redistributed secondary and tertiary clays and have been derived from the latter horizons. This view in regard to them was first expressed in regard to the so-called chalky clay of eastern England.

I prefer to call it the chalky clay, as Searles Wood named it, rather than the chalky boulder clay, because boulders in the true sense of the word, such as characterise the genuine boulder clays of North Britain, are infrequent in it. The term chalky applied to this clay depends on the fact that it is more or less crowded with chalk rubble and chalk fragments of various sizes, and that it has also incorporated in it a considerable quantity of chalk dust, whence its colour and superficial appearance. These peculiarities, which mark it over a wide area from Yorkshire to Finchley, and from Southwold to Warwickshire, are, nevertheless, a secondary, and not a primary, feature of the clay, and have disguised and confused the problem of its explanation.

Mr. H. Woodward says: "The occurrence of the chalk lumps in the clay may perhaps be accounted for in the following way. The surface of the chalk in Norfolk along the sea margin, and in places inland, is seen to have weathered in a very rubbly form to a depth sometimes of six or eight

feet. Frost and rains have no doubt been the chief influences in producing this; and this weathered chalk would furnish 'ready-made' the very material for the chalky boulder clay which consists so largely of lumps of chalk which have not necessarily been rounded by rolling" (*ibid.*, p. 123).

It has been noticed by several writers that while there is a common appearance to this chalky clay wherever found, due to the fact that it contains much débris of chalk strata, yet in regard to its other contents, and notably its matrix, it varies in accordance with the composition of the beds over which it lies, that is, with the substratum. This fact has been frequently noticed, and was, so far as I know, first observed by the Rev. W. B. Clarke, who, writing as far back as 1837, says: "The diluvial clay covers a great portion of Suffolk, Norfolk, Cambridgeshire and Essex, and at Cromer rises to 400 feet; much of it is yellowish, but the greater part blue. In both cases it contains chalk pebbles, sometimes in layers, but generally dispersed. This at once distinguishes it from the London and plastic clays." Mr. Clarke then goes on to argue that "the yellow clay was derived from the plastic, and the blue, from its peculiar fossils, from the clay below the chalk" (*Geol. Trans.*, 2nd series, v., p. 365).

These observations of a good geologist have been amply confirmed by later explorers. Thus Mr. Skertchly says, speaking of the Chalk, the Kimeridge and Oxford clays: "We find that the boulder clay lying upon these rocks partakes of their physical character. Thus, upon the chalk the boulder clay is very chalky, and, indeed, in some places, as at Mareham le Fen in Lincolnshire, and Thetford in Norfolk, it is almost entirely made up of that substance; at the former place it is quarried and burnt for lime, and at the latter the presence of seams of clay and ice-scratched flints alone enables us to discriminate between it and the chalk beneath. The Kimeridge clay is darker than the Oxford clay, and we accordingly find the boulder clay which reposes upon the former is darker than that which lies upon the latter. Where boulders are rare, it is sometimes very difficult to distinguish the boulder clay from the older rocks. The gault clay again takes the ground in but a small area in the Fens, but

the boulder clay 'picks it out' as it were, and at Modney Bridge brickyard, near Hilgay, for example, I have known the glacial bed to be mistaken for gault by persons quite familiar with the latter." "Similar remarks," says Mr. Skertchly, "apply to all other formations upon which I have mapped boulder clay. For example, the light blue upper lias clay of Leicestershire impresses its character upon the boulder clay which overlies it, and the other members of the liassic group, where they are in force, behave in a similar manner. The great Lincolnshire (inferior) oolite limestone around Melton Mowbray yields so large a quantity of material to the boulder clay there, that I have been in doubt as to whether the deposit might not be faulted limestone. These peculiarities are at once and correctly expressed by the statement that the ingredients of the boulder clay are, for the most part, supplied by rocks, upon or near which it reposes. That this is actually the case, and not an accident of colour, is further attested by the included fossils: *Gryphæa dilatata* and *Belemnites Oweni*, for example, are abundant upon the Oxford clay, and *Ostraea deltoidea* upon the Kimeridge clay" (Skertchly, communication to Geikie's *Great Ice Age*, new edition, p. 346).

Elsewhere Mr. Skertchly says: "The Kimeridge clay is easily recognised by its peculiar dark blue colour. Boulder clay over the area in which Kimeridge clay takes the ground is very largely composed of the latter, insomuch that, to take two cases far removed from each other, about Woodhall Spa and March, careful examination alone shows that it is a glacial deposit at all. . . . So too with the Oxford clay. It impresses its light blue character upon the boulder clay in a most striking manner, as may be seen wherever it outcrops. . . . In like manner, I might turn to the various formations over which I have mapped this interesting deposit, and show how in every case the boulder clay is made up of the wreck of the subjacent rocks."

The chalky character itself of the clay in certain districts is simply due to its being upon, or in close proximity to, the chalk *in situ*, and there is no justification for constituting the chalky clay a separate horizon. It is because the chalk occupies so much of the area, and is itself so easily dis-

integrated, that the name chalky clay, rather than oolitic clay or liassic clay, has been given to it.

The lesson I wish to deduce from these facts is that the chalky nature of this clay in certain places is no criterion of a separate origin and a separate history for the deposit. It means no more than that the same clay, where it lies on or near chalk, is chalky; where it lies on or near oolitic beds, is largely oolitic; and where it lies on or near liassic beds, is liassic. Where, again, it is remote from chalk, chalk débris is necessarily not present in it, and yet the clays may be, and probably were, distributed by the same forces, and were differentiated from each other only as the sandy deposits of one part of a bay are differentiated from the muddy deposits of another part of it. The fact is that the chalky clay, which, in the eyes of so many geologists, forms a deposit which is treated as *sui generis*, and as marking a particular horizon, is nothing of the kind, but, as I believe, is merely a local form of other clays occurring in eastern England, which do not contain chalk débris, but which resemble it in other respects, and which are, so far as we know, interlocked with it or mark the same horizon. Instead of there being several superficial clays in eastern England, whose various names, such as stony clay, or hessle clay, or purple clay, etc., suggest a varying origin, it is very probable there may be only one such clay, marked in different areas by necessarily different characters, pointing, not to a different date, but to different ingredients, and perhaps a different *provenance*.

Mr. H. B. Woodward, speaking of the constituents of the boulder clays of South Norfolk and of Suffolk, says: "Of these, oolitic material is conspicuous, and the great clay formations like the lias, the Oxford clay and the Kimeridge clay, appear to have suffered more in the formation of this boulder clay than the oolitic limestones which usually intervene between them" (*Glacial Drifts of Norfolk*, p. 117).

Again, he says: "In studying the junctions of boulder clay with various deposits on which it rests we sometimes find, as in West Norfolk, that on the chalk the line of demarcation is most obscure. It is difficult to say when the chalk ends and the boulder clay begins. In this way we may account for some of the very marly varieties of the boulder clay, which

in places are little else than ground-up chalk. The Rev. O. Fisher has spoken of such drift as an ancient manufacture of whiting 'on a magnificent scale'" (*ibid.*, p. 121). Quite so: but the whiting was manufactured, so far as we can see, before the drift was distributed, and was merely distributed with it.

The Rev. E. Hill says the matrix of the East Anglian boulder clay seems chiefly Kimeridge clay, while that of the Midlands is keuper and lias (*Geol. Mag.*, 1895, pp. 555-557).

In regard to the soft deposits over parts of London, Dr. Hicks says it is quite possible to mistake the brown clay which overlies the gravels where it happens to be free from stones for weathered London clay. He then goes on to say he had recognised this clay with "race" containing included patches of gravel in cuttings several feet thick in several places, and adds: "The presence of the gravel patches clearly proves that it is not London clay, *but a redeposited clay derived mainly from the denudation of the London clay.* This redeposited clay can generally be differentiated from the London clay, not only because it frequently contains 'race' and patches of sand and shows bluish streaks due to the percolation of water saturated with carbonate of lime, but also because of the manner in which it crumbles, when dry, into irregular fragments. London clay, even when much weathered, still retains evidences of lamination, and is easily split along well-defined lines. With the *remanié* clay, on the other hand, unless near the base, where it is usually more or less sandy, there is little evidence of lamination" (*Quart. Journ. Geol. Soc.*, xlviii., p. 460). Referring to the deposit in Gordon Square, he says he found in the gravelly clay many fragments and several well-preserved specimens of shells which had evidently been derived from the London clay. This find, he says, is important not only as proving that much of this yellow clay must have been derived from the London clay, but also as showing from the condition in which the specimens were found—their occurrence at one place only, and at that place in fair abundance—that they must have been transported.

A good proof that the boulder clays of Eastern Britain were of local origin and were not far-travelled is to be found when we examine the so-called glacial beds of the Nether-

lands, where similar foreign stones and erratics to those found in Britain occur, and where there are no boulder clays. If the latter had been far-travelled, and had come from where the supposed ice culminated, we should have had them present in the Netherlands as they are here. Here they were ready-made to its hand when the distributor of the drift acted upon it in the various secondary beds; there there were no such beds available, and the clay is largely wanting in the drifts.

Mr. Searles Wood, junior, says that the Hesbayen mud (argillaceo-sandy beds in Belgium) becomes on the extreme east of Suffolk more clayey, but yet less so than further to the west, and its included chalk débris are there, but scanty; while as we go westwards, and approach the region of the oolitic clays, from which so much of the argillaceous material of the boulder clay of the east of England has been derived, the clayey character of the deposit becomes more decided. Approaching the chalk district, as well as over it, the extensive intermixture of chalk detritus shows that the adjacent material largely contributed to the sediment, and that little or nothing was derived from the Hesbayen area (*Ann. Mag. Nat. Hist.*, 1864, pp. 12, 13).

In the west and centre of England, where the Kimeridge and associated clays are absent, we have the triassic clays and marls, which have been apparently the mother source whence many of the local clays have been derived.

Mr. Wyley, in describing a clay pit near Lappal Tunnel, mentions a curious intrusion of marl very like the ordinary red marl forming the upper part of the new red sandstone, but seen here among the coal-measure clays of the small spur of the clay field. A subsequent visit led to the conclusion that these marls were boulder clay, though largely derived from the red marl of the neighbourhood, which gives them very much the appearance of the red marl itself; but the intercalated beds of fine sand and seams of fine gravel, containing pebbles of syenitic, granitic and felspathic trap of various kinds, are foreign to the neighbourhood, though some of them may be secondarily derived from the Permian breccia of the Lickey or Clent Hills which is traceable to within a mile of the beds in question (*Proc. Dudley Geol. Soc.*, iv., p. 50).

Carvell Lewis, speaking of the so-called till at California, near Birmingham, says: "I find it simply the rewashed underlying Permian marl covered by torrential gravel. A local clay similarly made out of a mezozoic clay occurs at Rugby" (*Glacial Geology of Great Britain*, p. 64).

Deeley speaks of the clayey matrix of the boulder clay at Spondon, near Derby, as being formed chiefly of coal-measure clay with varying proportions of Keuper marl (*Quart. Journ. Geol. Soc.*, 1886, p. 448).

Speaking of the grey drift clays, Munthe says of the deposits in the island of Alsen: "An der genannten Landspitze erreicht der Geschiebemergel eine Machtigkeit von etwa 10 Meter und enthalt hier grössere und kleinere Partien von einen dunkeln fossilienführenden *Tertiärenthon*," etc. (*Studien*, etc., p. 54).

Another very important proof, as it seems to me, that the clays and sands which form such a large part of the drift beds already existed as clays and sands in tertiary times, and were only redistributed and not manufactured in the so-called glacial age, is to be found in the very familiar fact of the incorporation in the clays of great masses of undisturbed sands; and in the sands of masses of undisturbed clays, which have been taken up *en masse* where they lay and transported as boulders. A few examples will be very germane to this inquiry.

Dr. Ricketts has recorded some interesting observations on this subject. "Among the erratics in the boulder clay," he says, "may be included masses of unconsolidated sands and gravels often alluded to by local geologists as 'pockets of sand,' etc. The materials resemble accumulations situated in the bottoms of valleys as a bed upon which the boulder clay reposes. Their general shape is comparable to that of the section (representing masses containing a remarkable collection of dark green blocks of disintegrated traps, unmixed with other boulders) exposed in 1878 during the construction of the Bootle Dock. These were imbedded in a light green sandy matrix, and formed accumulations which were very conspicuous, the colour being in marked contrast to that of the boulder clay; their disintegration must have been due to the same causes as that of some

boulders of granite and trap to be alluded to hereafter. When brick-making was in progress behind the Mission House in Borough Road, Birkenhead, several blocks (five or six) of sand and clay were exposed; they contained a few erratic pebbles as well as bands an inch or so thick of vegetable mould. In one the clayey and carbonaceous beds were so doubled on themselves, being bent at the flexure, as to squeeze away the clay. In this case also the loose sand has fallen away and spread from the mass as it settled down in the boulder clay. One piece exhibited spots of carbonaceous matter which are probably rootlets of plants. Mr. D. Mackintosh has found plant remains in boulder clay near Crewe, and Mr. T. Ward of Norwich a fragment of wood obtained at a depth of thirty-five feet in undisturbed boulder clay. Blocks of the boulder clay itself, which would escape notice should they occur in the boulder clay proper, in consequence of its identity with them, have been frequently observed in those stratified sands and gravels which cover the bottoms of pre-glacial valleys, such as Happy Valley. Similar blocks also occur in the sandy boulder clay which immediately overlies these sands and gravels. Mr. A. Strahan, F.G.S., tells me he has observed them under similar conditions in other places" (*Quart. Journ. Geol. Soc.*, 1885, pp. 592-594).

Lamplugh, in a letter to the *Geological Magazine*, describes how the sandy patches in the boulder clay at Bridlington and Dimlington are clearly transported and derivative. He argues this from their not showing any signs of extension or regularity, at both places exhibiting the same kind of remnants, and the patches composed of many kinds of sand, gravel, silt and clay, some with shells of one kind and some of another, many with none, and all occurring at random in the boulder clay like boulders. Most of the patches are formed of materials not to be obtained on this coast at the time; coarse grains of greenish quartz form much of the sand, while the fine blue clays have in great part come from the waste of soft Neocomian beds. Great boulders of liassic chalk, with lines of black flints in it and of Neocomian beds, occur in the clays just like these "boulders" of shelly sand (*op. cit.*, 1882, pp. 383, 384).

In a later paper he speaks of the boulder clay at Bridlington as containing many crushed masses of sand, and sandy gravel and clay of all sizes and shapes, some as seen on the beach in horizontal section appearing more or less round and coherent, others occurring as long, thin, sometimes intermittent streaks between slabs of boulder clay, with every form between these extremes. The lithological character of the masses also varies, some consisting of clay alone, blue, brown or leaden hued; others of clay mixed with sand; others of gravel and sand with little clay. One of the largest of the masses consisted in part of roughish gravel, the larger pebbles being about the size of a small orange, thoroughly water-worn and rounded, and including black flint, red flint, nodules, some apparently Neocomian, others probably liassic, yellow clay stone, several varieties of basalt, brown quartzite, dark volcanic ash and other unknown volcanic rocks. Shells of *Tellina* with valves closed were found in the clay, and were filled with sand, none of which sandy matrix appeared outside the shells. The shells of *Pholas crispata* were generally found in a blunt cylinder of hard sand, which forms a faithful cast of the lower part of its boring. This cast often contained other shells, "and was, of course, a sample of the sea bottom on which the mollusc lived. The sand though lighter in colour was like that generally diffused through the clay masses." Mr. Lamplugh thinks the shells which lived in the sand bored down into the stiff clay, which again points to the clay before it was redistributed being of older date than the so-called glacial age. In regard to the general question of the derivative character of the included masses, he says: "The great similarity between the general aspect of the masses in localities so far apart as Bridlington and Dimlington; their ever-varying lithological character; their composition (which makes it improbable that they have been formed from the waste of rocks in the neighbourhood); the composition of the boulder clay in which they are contained, which is strikingly unlocal, its very flints being such as do not occur in the Yorkshire chalk; the marine débris so plentifully dispersed in the boulder clay—all point to the masses having performed no inconsiderable journey" (*Quart. Journ. Geol. Soc.*, 1884, pp. 314-318).

In a report presented to the British Association in 1888, Mr. Lamplugh, speaking of the Bridlington drift, says: "The inclusion of other beds is its most characteristic feature. In the section reported upon, these patches were of chalk rubble or of clayey silt and sand, but further east the lowest boulder clay included transported masses of Speeton clay mixed with some red chalk, while at Bridlington Quay and elsewhere some of the patches of clay and sand which it encircles are rubbly, fossiliferous, and form the deposit long known as 'the Bridlington crag'" (*op. cit.*, p. 9).

Lastly, Lamplugh describing the drift beds at Flamborough Head says that the till has at this place a curious and interesting layer of blue clay, which upon examination proves to have been chiefly derived from the Neocomian and Kimeridge clays, for which the nearest locality is the Vale of Pickering, seven miles distant. This is shown by such characteristic fossils as *Belemnites jaculum* of the Mid and Lower Neocomian, *B. lateralis* and *Exogyra sinuata* of the Lower Neocomian, etc., with nodules of weathered pyrites from the Upper Neocomian, and with a few small red chalk pebbles. Traces of the bed first occur at the Fog Gun House, and it may be followed for several hundred yards. In places it consists of pure Neocomian clay simply removed and in the condition of a boulder, but oftener it has a few red and white chalk pebbles worked into it, and sometimes passes into a kind of boulder clay. Near Selwick Bay it is ten feet thick. The chalk below it is marked by some slight crumplings.

Similar patches of travelled beds have been noticed in the boulder clay near Filey which are probably not so far from home, being derived from the Middle Kimeridge series. Mr. Jukes Brown seems to have looked on these patches of the Speeton clay as having been *in situ:* but this is not so. Even in an exposure *on the shore* near Filey the Kimeridge clay was surrounded by boulder clay. Speeton fossils are not very rare in the basement clay either at Bridlington or Dimlington (*Proc. Geol. and Pol. Soc., Yorks*, vii., pp. 244, 245).

If we turn from the boulder clays to the brick-earths, which, as we have seen, stand on the same horizon with them, we shall have a similar story to tell. These brick-

earths and loams, containing pockets of gravel and lumps of Sarsen stone and the bones of so-called pleistocene beasts, have all the appearance of being made up of local materials collected from the scouring of the adjacent wolds and downs, and of the fine marly contents of beds further north which have been washed. There is no sign anywhere that the matrix of these beds was the product of any grinding process during any glacial period, but on the contrary the loamy matrix of these brick-earths, etc., seems to me as old as the bones it contains, both bones and loam having already existed, when both were taken up and laid down again by whatever force it was that distributed them.

Let us now turn to the loess. Various theories have been proposed to account for the loess which explain it as a separate and substantive deposit. The earliest theory, which was first propounded by Bennigsen Förder, was that the deposit is of marine origin. This view is, I fancy, now limited to Mr. Kingsmill, and was entirely repudiated by every speaker when Mr. Kingsmill's paper was read at the Geological Society. The objections to it are overwhelming. In the first place, a mineral deposit formed of inorganic particles so heavily charged with a material like carbonate of lime could hardly be deposited from sea water. In the next place, the loess, although abounding in organic remains, has in no single instance yielded a trace of a marine organism: this objection alone is paramount. Thirdly, the loess is practically homogeneous and unstratified. It is true that concretions occur in it, as they have accumulated *in situ*, from the deposit of carbonate of lime about nuclei; but these have been formed in this way *in situ*, and are not evidence of stratification at all. Such a thing as *an unstratified* marine deposit 1,000 feet thick, quite homogeneous throughout, as is the case with the loess of China, is unknown among modern phenomena, and is *prima facie* impossible. Lastly, the postulating of a change of level of at least 6,000 feet in China, in quite recent geological times, and of a very extensive and widespread change in Europe also, is invoking a quite stupendous cause to account for what is much more completely accounted for by a much simpler one. On every ground we must conclude that there

is no evidence that the sea has ever overspread the two continents of Asia and Europe since, or concurrently with, the deposition of the loess, but, on the contrary, all the evidence points to the loess having been other than a marine deposit.

Having discarded salt water and its partisans, we must now consider the theories of those who make the loess a fresh-water deposit. Some of these argue that it is of lacustrine, and others of fluviatile origin. Both agree that lake and river, however, are merely instruments by which the loess was distributed, and urge that its *origin* is to be traced to the glacial mud formed by Alpine and other glaciers at the time when the ice age predominated. In this view all the partisans of fresh water are agreed. Lyell, Belt and Geikie, differing in other respects, are at one in this. Yet we may seriously ask on what possible ground, save a purely hypothetical one, this view has been maintained. The Alpine glaciers are no doubt much smaller now than they were in the great ice age, but otherwise they are doing precisely the same work, grinding the same rocks, and pouring out the same débris of denudation. Their great outlets, the Rhine and the Rhône in the north, and the Po in the south, are carrying seawards the same kind of matter in suspension that they formerly did. At all events, if this be not so, the burden of proof of showing the contrary is assuredly upon those who deny the fact. If this be so, how comes it that nothing like the loess is being manufactured now by the Alpine glaciers; that the sediment contained in the Rhine and the Rhône, except such part of it as is washed from the banks along which they flow, is other than loess? How is it that at Bonn the Rhine water contains no appreciable carbonate of lime, as Bischoff has shown? How is it that wherever we can trace deposits directly due to glacial action —*e.g.*, those being formed at the feet of the Greenland glaciers, and the great beds of materials which the glacial age is supposed to have left in various parts of North Britain and North America consisting of clays of various kinds—how is it, we say, that nothing in the shape of loess is to be traced there, but that these deposits in texture, composition and other characters are quite different from loess, properly so

called? Assuredly there can be but one conclusion, if we reason inductively, namely, that the loess is not a glacial mud at all, but had a very different origin. Let us now turn to the several theories of its distribution. First we will consider the lacustrine theory. This view was inspired by the limited area of the Rhine valley. It was suggested, in fact, that the loess which mantles that valley and those of its tributaries was deposited by a great lake, banked up by a barrier in the narrow gorge between Bingen and Coblentz. When, however, it was shown that the loess which exists at Basle and Schaffhausen would necessitate a dam 1,200 feet high to retain the waters of such a lake—and, further, that the lateral valley of the Lahn is itself filled with loess—the difficulty of postulating any sufficient barrier became obvious. But, apart from this, such a lake as was cited to explain the Rhine loess could not explain its distribution on the flanks of the Carpathians, on the great Polish plain, nor in China. Again, the contents of the loess are completely against a lacustrine origin. Although it abounds in shells, virtually none of them are lake shells; while the great number of débris of land animals which it contains precludes such an origin. Nor, again, can we understand such a vast *unstratified* deposit accumulating in a lake. The reasons against such a theory are paramount, and since Sir Charles Lyell wrote against it, it has been abandoned both in England and on the continent. We next come to Lyell's own idea, which he suggested in substitution for the lacustrine theory just quoted. This is still a living theory with some geologists. Lyell suggested that the loess of the Rhine valley is of fluviatile origin, and was deposited by the Rhine itself. This notion assuredly makes enormous demands on our credulity. First, the loess in structure is very different to the warp deposited by a river. Such warp is essentially stratified, and we can trace the annual layers by which it has been deposited; while the loess is several hundred feet thick, and throughout is homogeneous and unstratified. Secondly, it is so highly charged with carbonate of lime that it is incredible that a river should have deposited it unless its waters were actually saturated with that material. But, as we have said, Bischoff, in 1855, showed that the Rhine at Bonn had virtually no carbonate

of lime in it at all. Thirdly, the *contents* of the loess point strongly to its not being a fluviatile deposit. Sir Charles Lyell argues against its lacustrine origin, because the shells it contains are terrestrial and amphibious. Surely this very argument ought to be conclusive also against its fluviatile origin. The force of this argument is very great, for these shells do not occur locally and sporadically, but in great numbers, and characterise the loess in China and eastern Europe and America, as well as in the Rhine valley. Let us look more closely at their evidence.

Out of 211,968 shells from the Rhine loess examined by Braun, there was only one brackish-water form and three sweet-water forms; *Limnæus* and *Planorbis*, with but thirty-two specimens in all. Of the rest there were 98,502 examples of two species of *Succinea*, which is an amphibious species, and 113,434 specimens of land shells belonging to twenty-five species of *Helix, Pupa, Clausilia, Bulimus, Limax, Vitrina* (*Deutsch. Zeits. für die gesammten Naturwiss. Halle*, xl., p. 45).

In the Bavarian highlands Gümbel found one amphibious form (*Succinea*) and fourteen terrestrial ones, *Helix, Pupa, Clausilia* and *Bulimus*. Engelhardt, who has described the loess of Saxony, refers to twenty-four localities whence he has examined the shells of the loess, in which only land and amphibious shells are to be found, while in two only did he find the fresh-water form *Limnæus truncatulus*. Similar land shells are found in the loess of the Danube valley of lower Austria, Hungary, the Carpathians and Poland.

Mr. Belt tells us that he examined the loess in the valleys of the Rhine, Main, Danube, and the steppes of southern Russia, yet he had never seen a fragment of a river shell.

Baron Richthofen expressly says of the Chinese loess: "The land shells are distributed throughout the whole thickness of the loess; and their state of preservation is so perfect that they must have lived on the spot where we now find them" (*Quart. Journ. Geol. Soc.*, xxvii., p. 377, note).

The well-known French missionary in China, David, who has done so much for the natural history of that empire, speaking of the loess in the valley of the Yellow River and

in north-western China, says: "I never saw a trace of an aquatic shell in it, but merely the remains of certain small species of *Helices*, many of which exactly resemble those still living in the country". These shells, he says, are found at different depths, but chiefly in the upper part of the formation (*Quart. Journ. Geol. Soc.*, i., p. 93). We may take it, therefore, as an absolute feature of the loess that it contains virtually no river shells; for, as Mr. Belt says, the *Limnæus truncatulus* had its habitat most probably in small meres rather than in a river.

What is the inference from this? Why, surely, that the deposit which we are discussing is not a fluviatile deposit at all; for, as Mr. Belt says, all our larger rivers abound in shells. The shells of the loess are not only land shells, but they are shells loving the deep shade of trees and the damp recesses of woods, which woods could not exist where the banks of a river were continually depositing warp on a great scale. Nothing can be argued from the amphibious forms, the *Succineas*; for, as Lyell most frankly says, the shell is not strictly aquatic, but lives in damp places, and may be seen in full activity far from rivers, in meadows where the grass is wet with rain or dew (*Antiquity of Man*, p. 375). The other contents of the loess tell the same story; both the animal remains and the relics of man are most inconsistent with the fluviatile origin of the deposit. As we have shown, the remains of great beasts at Cannstadt, etc., occur in the loess as nearly as may be under precisely the conditions in which they are found in Siberia. We could imagine in a narrow, rapid river that carcases of mammoths, etc., might be borne along and be overwhelmed and covered with gravel and other débris if they stopped at any point and thus formed a barrier to the flow of the water, but how, in the case of a slow river—much slower than the present Rhine (which is Lyell's own postulate)—these remains as they are found at Cannstadt and elsewhere could be covered over and preserved in skeletons and in hecatombs, without dispersal by gradually accumulating annual layers of warp, is difficult to imagine. Why, again, primæval man should amuse himself by sowing specimens of his flint tools at intervals in the warp of the river, or in the river itself, is equally hard to realise.

If we turn from the contents of the loess to its distribution we shall be compelled to the same conclusion. In the first place, it occurs quite independently of the drainage of the country. It covers the plateaux as well as the flanks of the valleys. The river that could overflow these plateaux had surely ceased to be a river. How, by any mechanical theory, can we account for the water of a slowly moving river with a small fall, even with the greatest winter floods, depositing such masses of loess, not only 600 feet above its present level, but choking its own lateral valleys up to their very heads with it? What possible river could have deposited the loess of Poland, or the far more important loess of China, 6,000 feet above the sea level, or that of the United States? Assuredly, in every way we view the problem, the fluviatile origin of the loess seems incredible, and the difficulties of the explanation are overwhelming. Prof. A. Geikie, Mr. Belt and others have more recently than Lyell started another theory, which virtually amounts to a combination of the fluviatile and lacustrine views. According to them the waters of the Rhine and other rivers flowing northwards were dammed back by a *mer de glace* occupying the North Sea, and were thus made to overflow their upper reaches, like the Obi does now. This theory has all the objections which have been urged against the lacustrine and fluviatile origin of the loess, with some additional ones. How is the loess of the Danube valley and of Hungary, of the steppes of South Russia and Siberia, and more especially that of China, to be explained by such a bringing down of the glacial period almost to our own day? Where was the *mer de glace*, and where the rivers which were dammed back so as to deposit those vast masses of loess on the Chinese plateaux, 6,000 feet above the sea level? How, again, with the proposed *mer de glace* in the North Sea, involving a climate like Greenland or the Polar shores of Siberia, could the mammoth and his contemporary the hippopotamus, the *Ficus carica* and the *Cyrena fluminalis*, have lived in northern France and Germany? These objections seem to me overwhelming.

We are, therefore, bound to discard this modification of the older hypotheses, and to conclude that every theory

requiring a marine, lacustrine or fluviatile origin for the loess is inconsistent with the evidence.

In a former chapter I have argued at great length that the æolian theory of the loess championed by Baron Richthofen is not sustainable either. Nor can I any longer maintain a view of my own which I published at some length in the *Geological Magazine* many years ago, in which I tried to correlate it with the volcanic mud, of whose outpouring at various times we have abundant evidence elsewhere, although I am still disposed to think that the great quantity of carbonates with which it is charged may have been caused by those great outpourings of carbonic acid which Prestwich showed were coincident with the last important changes in the contour of the earth's surface.

I believe now that the loess cannot be treated as a distinct and substantive deposit and separated from the other drifts; but that it is merely the finest product of the same vast sifting process which the drifts went through in travelling southwards when they laid down their burden continuously, first depositing heavy boulders, then gravels, then sands, and lastly loams, of which the loess is only a modified form. I believe, further, that the loess in a different shape was ready-made like the other drift deposits were, and that we must trace it to those beds of fresh-water marl, of which such large débris still remain *in situ* both in Scandinavia and Germany, and of which the composition is in part so like that of the loess. *Inter alia*, these marl beds in Norway contain the same kind of queer-shaped concretions, called *puppchen* by the German geologists, and the same kind of threads of calcareous matter simulating roots. The disintegration of this finely levigated marl mixed with the finest particles of siliceous mud washed out of the other drift beds and charged with carbonates, due perhaps to the outpouring of carbonic acid, as I have mentioned, seems to afford us the most satisfactory clue to the origin of the loess. I shall have more to say of the loess presently. The point I wish especially to urge here is that its constituents were ready-made at the time it was distributed, and were not the product of the so-called glacial age, but date from tertiary times, and that it is quite continuous with the other drift beds.

Having considered the sands, clays and loams which form the greater part of the so-called glacial beds, let us now pass on to the coarser materials which have been assigned to the same period and are found associated with these sands, and let us first turn to the shingles and gravels of eastern England. These beds are often clean washed, and consist, with the exception of a few sporadic foreign stones, of smooth flint and quartzite pebbles of a lenticular or flat or oval shape, and sometimes, as in the case of the so-called cannon-shot gravel, of rounded flints of very considerable size. These beds occur in many cases as caps to the hills or on their flanks.

There can be very little doubt that these rounded and smoothed stones are the débris of marine and fluviatile shingles. It has apparently been argued by some that they were formed as pebbles during the so-called glacial period. I believe this to be a complete mistake, and, so far as my own observations go—and I have worked pretty hard among them—they all seem to me to be derivative, and to be the débris of disintegrated tertiary gravel and pebble beds. This view has been growing of late years.

Hutton, so far as I know, was the first to suggest that such gravels may have been the débris of the disintegration of older shingle beds. Thus he argues that "these water-worn materials had their great roundness from the attrition caused by the waves of the sea upon some former coast, and that, after having been thus formed by *agitation on the shores, and transported into the deep*, this gravel contributed to the formation of secondary strata," such as the pudding-stone he elsewhere described; and, lastly, that it was "from the decay and revolution of these secondary strata, in the wasting operations of the surface, that have come those round siliceous bodies which could not be thus worn by travelling in the longest river" (*Theory of the Earth*, vol. ii., p. 144, note).

Hutton's view is a general one, but in the particular case before us the case is still stronger than he urges. With the contour of the country as we know it there are no rivers in eastern England which either make or could make gravel or shingle, nor do we see whence the original stones could have been derived in the area named for fashioning the

pebbles. These pebbles are precisely like in form and character to the pebbles occurring in the Bagshot and Reading beds and in the Blackheath gravels. Where they occur in East Anglia, etc., in regular beds and undisturbed layers, as they do at Southwold and in other places, they are generally classed under the name of Westleton beds, etc., as of tertiary age. Where they occur in a disturbed condition and mixed with foreign stones, they add another instance to those already quoted of tertiary beds which have been *remanié* and redistributed by the same force and at the same time as the other so-called glacial beds were distributed.

The view that the pebbles in them are of tertiary age has been urged by Prestwich, and notably by Monckton, Herries and the other younger geologists who have done so much to unravel the history of the southern gravels. I would merely press their conclusions further, and affirm that all the rolled and rounded pebbles in the so-called glacial clays and gravels are derivative, and have been derived from earlier tertiary beds.

This view is strengthened by a comparison of them with the so-called angular drift. The angular flints which constitute this latter drift are frequently mixed with the rolled flints in the pebbly beds; but it is plain that they cannot both have been subject to the same triturating process, or they would have been rounded and worn down together.

Let us now turn to local and particular instances.

Mr. Horace Woodward writes that while much of Norfolk remained as a land area in later eocene and miocene times extensive sheets of loose flints mixed with loose matter would be found on the surface. The materials then accumulated may have been ready to hand for agents in later times to reassort and deposit elsewhere, if not in the same neighbourhood. Again, he says: "Scattered over the high grounds of Holt and Cromer, on the hills in West Norfolk, and in outliers at Strumpshaw and Poringland, are deposits of coarse flint gravel, containing large blocks of flint and sometimes paramoudras; and when I referred previously to the wear and tear of the chalk in miocene times as probably resulting in accumulations of flints, I thought then of these gravels, and have wondered whether they might not have been thus

derived and transported by glacial action, partly in the same way as some of the huge masses of chalk, as well as by floods resulting from the breaking up of the ice-fields" (*Scenery of Norfolk*, pp. 444-447).

The so-called glacial gravels of Essex, near Chelmsford, are described by Monckton as consisting (1) of flint pebbles probably derived from pre-glacial pebble gravel; (2) subangular flints, whose brown colour shows they have not been derived directly from the chalk, but from older gravels; (3) black flints from the chalk; (4) quartz pebbles of a white or fresh colour, derived either from older pebble beds or triassic beds; (5) pebbles of red and white quartz, etc., from the triassic beds of the north, the nearest point from which they can have come being Leicester; (6) blocks of quartz, etc., from the north; (7) and small pieces of chert originally from the lower greensand, but derived at second hand from older gravels. Of these he says (1) may well have come from the eocene or pre-glacial gravels of the immediate neighbourhood; (2) and (3) from the chalk or old gravels of North Essex; (4) and (7) from the pre-glacial pebble gravel (Westleton shingle) of North Essex (*Essex Naturalist*, v., p. 192). This shows what a large proportion of the contents of the southern gravels are derivative in the view of this excellent observer.

In another place the same writer, still referring to Essex, claims that while many of the pebble beds found there, and described as Bagshot by Searles Wood and the geological surveyors, are not so, they, in many cases, appear older than the boulder clay; but, he argues, they are not middle glacial gravels, for the beds are usually stratified and contain abundance of quartz, quartzite, etc. The materials, he adds, in all probability have been derived from the Bagshot beds; almost entirely so in such cases as the deposits at Holdenswood and Langdon Hill, but in other cases with an admixture of débris of glacial beds as at Norton Heath (*Proc. Geol. Assoc.*, ix., pp. 1-11). I would add that in Essex it is very probable that some of the shingle beds of the crag have furnished their contingent of pebbles to these mixed gravels.

Mr. H. Woodward speaks of the Buckinghamshire brick-earth as very closely associated with the clay with flints, and he says it may be considered as largely made up of the

wreck of eocene strata commingled with accumulations of clay with flints (*Geol. Mag.*, 1891-92).

Murchison says that some of the débris in the holes and crevices of the white chalk may be found to actually belong to the tertiary period. He further identifies the combe rock with the flint crag of the Weald, which differs from it only in containing disintegrated débris of the lower greensand instead of tertiary beds. Speaking of the drift near Brighton, he says: "On a first inspection geologists might doubt whether these were really strata of the age of the plastic and London clays *in situ*, so completely is the mineral character identical, while their eroded upper surface, on which the chief mass of the flint breccia is placed, would seem to separate the two deposits. But both these signs are fallacious, for in the very heart of these clays and sands I found fragments of *Mytilus edulis* and other sea shells of existing species with their colours preserved, and among them three or four perfect specimens of *Littorina littoralis*. . . . It thus follows that this detrital deposit of Brighton, however diversified in aspect, and whether it be called combe rock, elephant bed or brick-earth, pertains to one and the same group, and is referrable to so modern a geological era that the sea shells now living then prevailed, although the great land animals which also then lived have long ceased to inhabit the country."

In describing the angular drift on the South Downs, west of Brighton, Murchison speaks of its matrix as "highly ferruginous loam and stiff clay, evidently the remains of the plastic clay," while in regard to the coast flats of Little Hampton and Bognor he says it has precisely the same origin as the angular breccia of the hillsides; the plain of rich arable land is chiefly composed of the breaking up of the plastic clays and sands and of the London clay, mixed together irregularly and interspersed with angular flints. Murchison goes on to point out that at Clapham Common, east of Patching, a mass of the undisturbed Woolwich beds still remains *in situ* just as it was originally aggregated on the surface of the chalk, and unmixed with angular flints (see "Flint Drift of South-Eastern England," *Proc. Geol. Soc.*, 1851, *passim*).

Mr. H. J. O. White says: "The materials of which the so-called glacial gravel is composed are derived from two distinct sources: the flints in all states, the fragments of Sarsen stone and iron-stone, and the smaller quartz pebbles, either directly from the cretaceous and eocene beds, or from the older drifts of adjacent areas; the red quartzites, grits, sandstones, blocks of vein-quartz, etc., from the trias of the Midlands. Doubtless the two sets of materials were already mingled when they reached this district; but it is interesting to note the increased size and number of the foreign constituents to the west, in the neighbourhood of Goring. The converse of this, *viz.*, the easterly decrease in the proportion of red quartzites in the gravels of the Thames area, is noticed by Dr. Buckland in his *Reliquiæ Diluvianæ*, p. 252" (*Proc. Geol. Assoc.*, xiv., p. 27).

In discussing the surface pebble gravels of the south of England, Monckton and Herries describe them as in the main reconstructed Bagshot and Bracklesham beds which have been redeposited and formed a new pebble bed at the surface of the ground. In certain cases where northern drift existed close by, foreign pebbles found their way into the rearranged beds. In other cases where it was not thus available these rearranged beds are distinguishable by their materials being deposited in a much more irregular way. They mention their occurrence in several places as between Pirbright and Ash, at Hook Heath, at St. Anne's Hill, the plateau west of Egham, Langdon Hill and Holden's Wood. In the valley of Virginia Water they describe the pebbles in the gravel as for the most part derived from Bagshot beds, while the angular flints have come from the plateau gravels (*Proc. Geol. Assoc.*, 1889, p. 1, etc.).

In a paper on Bagshot pebble beds and pebble gravel Mr. Monckton cites some deposits which exist at Park Hill, Croydon, and at Wellington College, etc., as débris of portions of the pebble bed which, together with the sandy strata in which it was imbedded, has been destroyed by denudation, the sandy and clayey portions of the bed having disappeared, and the pebbles having been left to form a new and irregular deposit along the present surface of the ground. He argues that all over the Bagshot country in the London basin on

high ground are deposits sometimes called Bagshot and sometimes pebble gravel or gravel of doubtful age. They are discriminated generally by the fact of some of them containing quartz, angular or subangular pebbles and coarse ironsand mixed with the regular rolled flints of the Bagshot age. He looks upon these extraneous elements as having been introduced when the beds were rearranged, and the gravels as being substantially derived from Bagshot beds. He enumerates several instances of these resorted and rearranged gravels, and urges that in them the rounded pebbles have come from true Bagshot or Bracklesham pebble beds and the subangular flints from the plateau gravels.

In the Hertfordshire hill gravels, Hicks tells us, are many well-rounded pebbles from the tertiary beds, but the majority of the flints are subangular. A few pebbles of quartz and quartzite are found and also fragments from the lower greensand.

Judd ("Geology of Rutland," *Geol. Surv. Mem.*, 1875) describes some gravels, sands and brick-earths in that county as pre-glacial; they contain local rocks only.

In Aveline's *Geological Memoirs* of the country round Nottingham the gravels and sands in the drift are described as largely derived from the Bunter pebble bed.

In regard to the beds of flint and chert detritus near Lyme Regis and Axminster, the bulk of the material is local and sometimes, but slightly, reassorted, but rolled fragments of grit and quartzite and pebbles of quartz, some of large size, are met with. These foreign pebbles were noticed by Buckland in 1823. The foreign water-worn pebbles, in Mr. Ussher's opinion, may have resulted from a redistribution of tertiary gravels (Horace H. Woodward, *Geology of Lyme Regis*, xlviii.).

The most interesting of the derivative pebbles in the gravels of southern and eastern England are those derived from the break-up of the triassic beds of Warwickshire.

These pebbles were well described by Buckland. He says: "The new red sandstone formation in the central parts of England contains an enormous deposit of pebbles of compact granular quartz, forming large beds, which may be seen at Bridgenorth, Lichfield and Birmingham. They constitute a

regular stratum, subordinate to the red sandstone, having been reduced to the state of pebbles by the action of violent waters at or before the time of the deposition of this formation. From this lodgment in one of the deep and regular strata of the country enormous quantities of the pebbles in question have been swept away by the diluvial waters, and dispersed superficially over the adjacent districts and midland counties, without any reference to the rocks they lie beneath, and mixed with fragments of other rocks both older and younger than the red sandstone. They have been collected in prodigious numbers along the plains subjacent to the escarpment of the oolitic limestone that crosses Warwickshire, near Shipston-on-Stour, particularly on the south of that town at the base of Long Compton Hill. They are there accompanied by pebbles of white quartz, lydian stone, gneiss, porphyry, compact felspar, trap, sandstone of several kinds, lias, chalk and chalk flints.

"Between Shipston and Moreton in the Marsh they have been drifted into a kind of bay, formed by the horn-shaped headland of the Campden Hills, which project like a pier-head some miles beyond the ordinary line of the great limestone chain of the Cotswold Hills. The mouth of the bay opens directly to the north-east, from which quarter it is probable the current which brought the pebbles in question had its direction, for on the south-east of Shipston there are pebbles of a hard red species of chalk which occurs not infrequently in the Wolds of Yorkshire and Lincolnshire, but is never met with in the chalk of the south or south-east of England. The nearest possible point, therefore, to which these pebbles of red chalk can be referred is the neighbourhood of Spilsby in Lincolnshire, whence a diluvial current flowing from the north-east would find an unobstructed passage across the plains of Leicestershire to the Bay of Shipston and Moreton in the Marsh. With these pebbles of red chalk are others of hard and compact white chalk, such as accompany the red chalk in the two last mentioned counties, and which occur also at Redlington in Rutlandshire.

"The diluvian current thus emptied into the Bay of Shipston from the north-east appears to have continued its course onwards beyond the head of this bay, near Moreton in the

Marsh, bursting in over the lowest point of depression of the great escarpment of the limestone, and, being deflected thence south-east by the elevated ridge of Stow in the Wold, to have gone forward along the line of the Evenlode by Charlbury, till it joined that of the Thames at Ensham, five miles south-west of Oxford."

Conybeare tells us that in Leicestershire "chalk, flint and oolitic boulders chiefly abound, and next to them in quantity are the granular quartz-rock pebbles resembling those from the Lickey, with others of white quartz". This is apparently in the chalky boulder clay. These quartzite pebbles also occur in Whittlebury Forest, near Northampton, and at Brackley. Sedgwick mentions similar quartzite boulders among the foreign stones found in the drift of Cambridgeshire.

In regard to the source of these quartzite pebbles I shall quote from an admirable paper upon them by Mr. W. J. Harrison in the *Proceedings of the Birmingham Philosophical Society* for 1882. He says:—

"Pebbles of quartzite, comparatively small in size and few in number, occur in the drift along the northern edge of the Thames valley; they may be seen, for instance, in the great chalky boulder clay at Finchley. Tracing these pebbles northwards, we find them increasing in numbers in the surface deposits, and increasing too in bulk, until we reach the counties of Worcestershire, Staffordshire and Nottinghamshire; further to the north and west they are comparatively rare or absent" (*op. cit.*, p. 157).

Again, he says: "It seems perfectly clear that the pebbles occurring so abundantly in the drift of the Midland counties were derived from the pebble bed or conglomerate which forms the middle member of the Bunter sandstone, or lower trias, and this for several reasons:—

"1. The pebbles in the drift are absolutely indistinguishable lithologically from those in the Bunter.

"2. Certain fossils occur in both, and when the conglomerate has been more perfectly examined, it will probably turn out that all the fossils of the drift quartzite pebbles occur also in the Bunter.

"3. The Bunter pebbles are often cracked, and often bear

abrasions caused by internal pressure of the pebbles in the beds. The same impressions and the broken pebbles also occur in the drift. Near Birmingham the number of pebbles showing these marks is very considerable. As we go south the impressions become lost to the eye, but can still be felt; further south, again, they entirely disappear."

These Lickey pebbles are very widely distributed.

Dr. Buckland says he had seen the quartzite pebbles on the summit of the chalk hills round Henley, Maidenhead, High Wycombe and Beaconsfield, and that they had been noticed by Lord Grenville in his park at Dropmore and in the gravel pits at Burham. In all these places the great mass of the gravel is composed of imperfectly rolled flints, derived from the neighbouring chalk, mixed with rounded oolitic gravel; also in the valley of the Cherwell and the plains adjacent to it, from its source at Claydon and Cherwelton to Banbury and Oxford, *e.g.*, at Steeple Aston, Heyford, Rowsham, Kirtlington and Kidlington. At Abingdon they occur not only in the gravel beds of the valley, but are scattered loosely over the plains, composed of various strata, around that town as well as on the low hills round Newnham, Dorchester and Wallingford. Among these pebbles, especially at Abingdon and in Bagley Wood, there are many of porphyritic greenstone and greenstone slate which cannot have come from any nearer source than Charnwood Forest in Leicestershire. They occur on the top of Henley Hill, on Ammor Hill and the highest summits of Witchwood Forest.

Buckland also speaks of finding these stones in the gravel pits of Kensington and Hyde Park.

Mr. Monckton gives a section of a gravel pit at Reading in which they occur, while they are also found in the gravels of Kingsclere with Sarsen stones and at Newbury with animal remains (*Geol. Trans.*, 2nd series, vol. ii., p. 128).

While we thus find that the gravels of Central and Southern England were supplied with larger quantities of ready-made pebbles from the Lickey beds at the time when the drift was distributed, another considerable stream was distributed in the gravels of the west of England from the disintegrated pebble beds at Bovey Tracey.

Leaving England, Mr. K. Martin, of Leyden, speaking of

the boulders in the beds in the neighbourhood of Osnabruck, says: "In dem ganze Gebiete eine Masse von Lias und Dogger geschieben mit deutlichen und bestimmbaren Versteinerungen sowohl in den *Tertiarablagerungen* als in der Diluviengeröllen gefunden werden" (Erstes Jahresber. des Natura-Vereins von Osnabruck, 1870 and 1871, p. 24). Martin adds: "Ich selbe sah beim Herrn Director Holste von Georg Marien das Bruchstück eines *Ammoni capricornus*, welches aus der Tertiär von Bersenbrück stammte".

Buckland says pebbles like those from the quartzose rock of the Lickey have been found by Strangways mixed with the diluvium of large boulders at Petersburg and Moscow. He found similar rocks at Olonetz, on the Ladoga lake, and in Catherinenburg. The rock of Olonetz is probably of the same age as the Lickey beds. "I have myself found them, between Basle and the south extremity of the Vosges, scattered over the low hills on the east of Altkirk. These were probably derived from the neighbouring mountains of the Vosges and Black Forest" (Buckland, *Geol. Trans.*, v., p. 515).

In America the problem before us is not so easy to illustrate, since the uniformity of the subjacent beds is very great, and many of the American geologists do not seem to have asked themselves as a preliminary question whether these great masses of sand, clay, shingle and gravel are not merely rearranged materials dating very largely, if not entirely, from tertiary times and not the products of the so-called ice age at all. We have phenomena so similar there, however, to those of Europe that we can hardly doubt they ought to be explained by the same cause, *e.g.*, the masses of quartzite shingle or quartzite rolled drift which has come not from the Laurentian highlands but from the Rocky Mountains, and which so resemble our own shingle and pebble beds (see G. Dawson on the "Superficial Geology of the Central Region of North America," *passim*). He also mentions how the drift changes in colour and texture with the ground it covers, as if it had incorporated pre-existing materials. Thus, he says, ascending to the plateau level from the extreme western point of Lake Superior by the Northern Pacific Railway, the drift is seen to have a reddish-purple colour, which continues,

though gradually becoming less marked, for some distance after attaining the summit. The colour then changes to the pale yellowish grey which is generally characteristic of the drift of this plateau. The red drift is derived from the red rocks of the borders of the lake (*i.e.*, Lake Superior) and is found along its southern side. It is here bounded by a line lying a short distance back from the north-west shore and nearly parallel to it. This western edge of the red drift has been already noticed by Whittlesey in his paper in the *Smithsonian Contributions* (*op. cit.*, p. 609).

In his paper on the glacial deposits of south-western Alberta, G. Dawson speaks of the rolled gravels and associated clays and sands which underlie the lower boulder clay, and are known as the Saskatchewan gravels. These Saskatchewan beds, which lie below the drift, have been identified by some with the Lafayette gravels. Dawson further says that "as the boulder clay is followed eastwards it gradually changes into the typical Saskatchewan gravel, in places associated with silty or sandy beds". Speaking of the lower boulder clay, he says "the pre-existing Saskatchewan gravels have evidently become incorporated with it in places to an unknown degree" (*ibid.*, p. 59). These Saskatchewan gravels contain materials which have travelled a long way from the mountains.

In a paper on the pre-glacial decay of rocks, Mr. R. Chambers specifies the beds beneath the boulder clay in various places in Canada, etc., whence the drift materials have no doubt been derived. Thus at St. George, on the east bank of the Chaudière river, where the boulder clay is sixty feet thick, he describes the subjacent beds as (1) stratified sand and clay, called by the miners quicksands and pipe-clay; (2) coarse stratified gravel with pebbles and a few boulders; (3) a local deposit of coarse, slaty materials, apparently decomposed quartzose slate; (4) fine-grained yellow sand with ochreous streaks; (5) the last graduates into rotten rock *in situ*; (6) slates and sandstones, decaying, fissile and non-glaciated.

Again, in a pit in the valley of Rivière du Loup, under a thickness of seventy-five feet of boulder clay, he mentions (1) a tough, dark grey stratified clay, one to three feet thick;

(2) grey ochreous sand stratified (quicksands of the gold miners), twelve to fourteen feet thick; (3) compact unstratified clay (pipe-clay of the miners), six feet; (4) grey stratified gravel with numerous pebbles and a few waterworn boulders, five feet; (5) hard, yellow, oxidised gravel, stratified, with an abundance of worn boulders, from two feet in diameter downwards. None of the materials are glaciated, and all are of local origin. Thus we have here below the boulder clay forty-five feet of assorted waterworn stratified deposits resting upon denuded beds of rotten rock and non-glaciated rock surfaces. Mr. Chalmers says of these beds "no fossil remains have yet been detected in them, *but they are probably of tertiary age*". He compares them with similar beds under boulder clay at Brandon, Vermont. "At the Little Dillon river they lie immediately on the rough, broken, jagged surfaces of the non-glaciated slates. . . . Local sheets of these decayed rock materials were first noted in the maritime provinces of New Brunswick, Nova Scotia and Prince Edward Island, resting on carboniferous rocks, and have since been found in almost every part of the higher and lower grounds, and everywhere overlaid by boulder clay, and it appears that from pre-pleistocene times a thick continuous sheet existed in some places, as in Prince Edward Island, exceeding the boulder clay in thickness."

Chalmers points out how in the maritime provinces these beds are found to contain the same angular pebbles or gravel as the drift above them, and how they had been shifted greater or less distances from their original position, and the interstices filled with a bluish or grey clay, or sandy clay, which beds graduate upwards into true boulder clay. He continues: "The occurrence of such extensive sheets of decomposed sedentary rock in the region under consideration, much denuded as they seem to be, points to the former existence of a universal mantle of this material overspreading the country everywhere in tertiary and preceding ages. . . . During the glacial period this decayed rock furnished the principal portion of the material constituting the boulder clay, and also that of the overlying, modified, post-glacial beds" (*Brit. Assoc.* (Toronto Meeting), p. 273, etc.).

Having analysed the evidence of the gravels and the shingles which occur so frequently among the glacial beds, and shown how, where we can trace their history, they are formed of derived materials, let us now turn to the larger boulders, for which an *a priori* case of the same kind seems almost conclusively provable.

The boulders of the boulder drift consist of two different categories—those which are rounded and have their rounded surfaces polished and smoothed, which form a vast majority of those that are far travelled; and those which have angular contours and are only slightly rubbed and eroded.

The former class are almost universally treated by continental glacialists as water-worn and water-rolled, and among the Swedish geologists the beds formed wholly of such materials are known as *Rullstenbildningar*. It seems impossible to attribute them to any other cause than that of water rolling. We cannot understand how ice in any shape or form could possibly manufacture such rounded and rolled stones as these are. Glacier stones, when formed by rubbing under pressure between the ice and its beds, assume the shape of flat stones with parallel faces, like the skids of a coach, and have been styled slipper stones. They differ entirely from these rounded boulders.

Among the glacial geologists it has been the fashion to treat these rounded boulders not as directly made by the ice, but as indirectly produced by being rolled in sub-glacial streams, such as we see under Alpine glaciers, where a limited number of such stones no doubt are found.

This in the case of the drift seems quite a fantastic notion for many reasons. In the first place, they are found not arranged in long ribbon-like defined beds and lines of deposits like the beds of streams, but in continuous mantles, spread over the whole country without a break and covering hill and valley alike, quite irrespective of the drainage of the country. Unless we are to postulate that the supposed ice-sheets were drained, not by occasional sub-glacial streams, but by a whole continuous flow of water continually moving underneath their whole bulk—unless we are, in fact, to postulate that the ice-sheets rested on widespread sheets of water, and were mere glorified icebergs—we cannot explain

how these boulders could have been made and deposited in this fashion.

Again, in sub-glacial streams, such as we know them in the Alps, the water washes away the sandy and clayey matrix and leaves a deposit of clean boulders or gravel in its bed, while in the drifts the stones are imbedded in a continuous matrix of clay or sand.

Thirdly, if we map out the lines along which these rounded boulders must have travelled on their way from their first home, where the mother-rock whence they were originally detached is situated, and try and realise what manifold hills and dales these lines traverse, we shall be constrained to conclude that no streams or rivers or sub-glacial sheets of water could possibly have flowed in the ordinary course of things and carried them along unless in those days water could run up and down hill galore.

Fourthly, it is not enough that these stones should be rolled along in order that they should become rounded; they must be roughly and rudely rolled against other stones, or against the rocky beds of the streams or of the strata along which they travelled, which must have been at least as hard as themselves. Ice or water alone would not smooth and round and polish them; nor would rolling them along beds of clay and sand, where they would be wrapped round and well padded on all sides by a soft cushion. It must be remembered that in Northern Europe, for instance, the whole line of country over which they must have travelled from their first home in the solid beds of rock, was, for the most part, covered with quite soft materials, such as disintegrated sand and clay, and, where hardest, no harder than chalk. It seems to me as clear as can be that it would be absolutely impossible to manufacture rounded boulders of granite, gneiss, greenstone and basalt by any such process, however far the journey they made. Hence it seems to me incredible that these rounded boulders have been made either *in situ*, or during their journey from their original rock beds by any form of ice. There is no mill available for such grinding as this, either among the chalky clays of East Anglia, or the red sands and blue clays of Lancashire, or the geest and brown sands and clays of the great German plain,

or the loess and *tchernozem* of Russia, or the corresponding beds of North America.

The *prima facie* case here stated seems to be overwhelming. It can be supported in other ways. Thus in a great many cases the rounded foreign boulders are mixed in the same beds with local angular drift. If the boulders in the boulder beds had been made *in situ*, it is incredible how this could be. The angular drift is not formed of harder, but, if anything, rather of softer materials, and it is ridiculous to suppose that the force which caused the rolling and shifting of the materials could or would discriminate in this arbitrary fashion, and roll some of the stones and leave the rest in their primitive unrolled angular forms. It seems, on the contrary, that whatever it was that distributed the drift brought the rounded and smoothed foreign boulders with it ready-made and mixed them with the angular débris on the spot.

This is by no means a new view. Thus Godefroy was of opinion that even the materials of the moraines of Switzerland have not been derived by the glacier from the solid rock, but from the re-arranged portions of a great pre-arranged diluvial deposit which had accumulated in the radiating valleys in a period of great disturbance anterior to the existence of the glaciers. Describing a tract of fifteen leagues he infers that such a mass of boulders could never have been deposited by a glacier proceeding from mountains of no greater altitude than the Alps (*Proc. Geol. Soc.*, iii., p. 673).

Direct evidence that the force which moved the boulders found them ready-made is not always forthcoming, for the question does not seem to have been often raised; but there is some evidence notwithstanding that in some cases they were so brought, leaving it a reasonable inference that this was universally the case. This evidence is of two kinds: first, of boulders showing traces of having had their surfaces weathered before they were transported, and, secondly, in the case of such stones as limestones, evidence that after they had been made into boulders they were pierced by molluscs, etc., when they were lying under the sea. I will quote some instances of both kinds.

In the report of a committee of the British Association for 1888 on an ancient sea-beach near Bridlington, there is an

account of a deposit resting on the terraced "scaur" of the chalk composed of water-worn chalk pebbles of all sizes from fine gravel to large stones over a foot in diameter. Many of them are perforated by the borings of *Pholas*, *Saxicava* and *Chione*, which shows that an uncovered scaur of chalk has extended at least to near low-water mark. There were also pebbles of grey flint—some rather larger in size, which must have drifted four or five miles—and also some foreign pebbles, the largest found being a boulder of basalt 12 × 5 × 3 in., and another of porphyrite 7 × 4 × 3 in. The most numerous of these strangers were pieces of brown or black laminated bituminous shale; other pebbles are of basalt, granite, quartz of various colours, porphyrite, etc., the whole, say the committee, forming an assemblage not strikingly different from that of our glacial beds. Some of the pebbles are well rounded and others subangular.

The bed in which they occur, according to the committee, underlies the oldest boulder clay known in Yorkshire, and they style the bed *sub-glacial*. The reporters refer to the occurrence of similar travelled pebbles in the Norfolk Forest bed series ("Report on Bridlington Beach," *Brit. Assoc. Rep. for* 1888, pp. 1-11).

Dr. Ricketts refers to boulders which have been exposed to other influences before they were floated away and dropped into the clay. "Some of granite," he says, "are weathered all over, their entire surface being roughened and so far disintegrated that fragments can be broken off by the fingers. Others in the same state have also had a portion split off. Some, having their surfaces glaciated, crumble into fragments by slight pressure; while others cannot be removed without separating into their component crystalline particles, though when *in situ* each granule retains its relative position, and a careful removal of the clay may even show their surfaces to be smoothed and polished. A somewhat similar weathering is often observed in various kinds of volcanic rock. In some it peels off in concentric laminæ. Where this disintegration has not penetrated the whole mass, the central nucleus remains solid and unaffected. Examples of a different kind are frequent in which the mass is disintegrated throughout, being easily crushed or broken and the granules separated.

"Mr. J. H. Kinahan, of H.M. Geological Survey in Ireland, has informed me," says Ricketts, "that blocks of disintegrated granite are frequent in the glacial deposits, especially those of Wicklow and south-east Wexford, being more prevalent in what he calls moraine drift than in the boulder clay. There are boulders imbedded in moraine accumulations—near Shap, in Westmoreland—which have become disintegrated in various degrees, and to an extent as great as those occurring in the boulder clay of Cheshire, whereas at the present time the Shap granite, both in the well-known blocks and *in situ*, weathers only on the exterior.

"What Mr. Kinahan states regarding the granite boulders of County Wicklow is equally applicable to some of volcanic origin in the glacial deposits of County Antrim, where they occur in every stage of disintegration, but modified according to their structure. Rocks of various kinds are coated with a powder derived from their disintegration, generally of a light green colour, having mixed with it minute fragments of the same. In some, the general surface of which affords proofs of glacial erosion (? glacial, H. H. H.), channels or hollows have been formed subsequently, these being filled with similar disintegrated materials. In other instances the weathering has so far extended throughout the whole surface that on removal they break up entirely.

"A large proportion of carboniferous-limestone pebbles bear evidence of atmospheric and chemical erosion in a variety of ways. Occasionally they are weathered all over, and portions of organisms stand in relief; more frequently they are eroded in the same way over a considerable surface, while the remainder continues intact with its ice-marks unaffected.

"A frequent feature is the formation of channels or hollows in the blocks. This occurs without affecting other portions which may still retain marks of glaciation (? of erosion, H. H. H.). In many instances limestone pebbles have been split into fragments which are occasionally in apposition. A glaciated one found *in situ* in the boulder clay is split into four fragments which remain in apposition; the split surfaces, as well as the outer portion close to them, have subsequently been somewhat eroded since their fracture.

"Several pebbles of limestone are not only glaciated, but

also perforated by mollusca and sponges. As a rule no shells are retained in the cavities. There is generally, if not always, evidence that the borings have been made subsequently to glaciation (? rather to erosion). In some instances they have afterwards also again been exposed to glacial friction (? say rather friction), and fragments have likewise been broken off prior to their deposition in the clay. In two instances glaciated (? eroded) and perforated blocks were found to be afterwards weathered, one over the perforated surface, the other as a channel-like groove on the portion covered with striæ. A solitary example of boring in softer limestone (it may be of Antrim chalk) contains many shells entire, and its surface is covered with *Serpulæ*."

Dr. Ricketts then goes on to refer to the curious and fantastic shapes into which some erratics have been weathered owing to differences in their texture, and of which he figures some specimens. These must have been so weathered before deposition. Several blocks subsequently to weathering have had portions of their surface rubbed.

In reference to these facts Dr. Ricketts continues: "The peculiar instance of weathering which rocks of different kinds have undergone prior to their deposition in the boulder clay appears to have escaped notice almost entirely: these are too conspicuous to be overlooked". In answer to a question from Dr. Hicks, the author of the paper said that the evidence was entirely in favour of the decomposition having taken place before the imbedding of the fragments (see *Quart. Journ. Geol. Soc.*, 1885, pp. 594-598).

Dr. Ricketts elsewhere refers to his having found a very large number of erratic boulders, some of which were sculptured by the action of the weather, and bore no traces of what he calls glaciation or rolling, while others with a greater or less amount of ice polishing and scratching (? ice, H. H. H.) are also distinctly weathered. Again, speaking of the flints he had found in the Lancashire drifts, he says of some of them: "They have been so altered by weathering that they are white throughout their whole substance; one has a fragment of white limestone attached. This," he says, "has a granular appearance; but although thus altered, its hardness is such that it can only be referred to the chalk or white

limestone of Antrim. The flints weathered but not water-worn coincide with those so abundant on the eroded surface of the chalk of the north of Ireland. Mr. Isaac Roberts possesses a specimen of flint exactly similar to those which at Belfast are situated in the hollows found on the eroded surface of the chalk, and lying immediately under have been altered by exposure to the action of the basalt" (*Proc. Liv. Geol. Soc.*, 1876-77, pp. 19, 20).

Mr. T. Sheppard in describing the gravels of Holderness containing far-travelled erratics of primitive rocks, says several of the boulders are well-rounded and bored with *Chionæ* and *Pholas crispata.* The holes in the pebbles occasionally contain the shells in a perfect state of preservation, though sometimes sand takes the place of the shells. Other chalk pebbles are cracked from their centre outwards. The boulders of gneiss and schist are occasionally very rotten, breaking into powder at the stroke of the hammer, showing they were weathered before being deposited where they are (*Proc. Yorks. Geol. and Pol. Soc.*, p. 5). In regard to the organic boulders in the same deposit, he makes the interesting remark that he found a radius and ulna of a *Bos taurus*, inside which was a small quantity of light-coloured plastic clay totally different from the neighbouring boulder clay (*ibid.*, pp. 9, 10). These facts point to the boulders just described being derivative. Mr. Sheppard says in a note: "Of course some of these boulders were probably scattered over a sea-bottom before being picked up by the ice-sheet" (*Quart. Journ. Geol. Soc.*, xlvii., p. 410).

Speaking of the boulders found in the Bridlington boulder clay, Lamplugh mentions the occurrence in the basement clay of numerous blocks of *various kinds of rock* bored by *Saxicava*, *Pholas* or *Chione*, the borings in the former of these cases sometimes still containing the shells and foraminiferous sand. "I also think," he says, "from the well-rounded and water-worn appearance of most of the smaller boulders and pebbles in the basement clay, that most of these have suffered aqueous erosion at some period of their history after their detachment from their parent rock and before their incorporation in the clay, and that they are the relics of an old shore line," and he, consequently, among other reasons, con-

cludes that the beds are not in place (*Quart. Journ. Geol. Soc.*, 1884, p. 317).

Again, writing in 1889, he says: "The presence in the boulder clay of well-rounded beach pebbles and of *Pholas* and *Saxicava*-bored limestones proves that many of the erratics must have been for some time on a sea-bottom" (*Proc. Yorks. Geol. and Pol. Soc.*, xi., p. 290).

Carvell Lewis says: "On the road from Ardfert to Ballyheige, two miles north of Ardfert, rounded boulders of limestone begin to appear in quantity. Most of these boulders are curiously eaten in short, curved, worm-like burrows, as if by a marine worm or mollusc, indicating that they have been in the sea" (*Glacial Geology of Great Britain*, p. 131).

In speaking of the basement clay of East Yorkshire, the same writer says: "The carboniferous limestone boulders in the basement clay are smaller and more rounded than in the purple clay; and Mr. Lamplugh told me they are often perforated by marine mollusca, as if they had been lying on the sea-bottom before the ice-sheet advanced over them and took them up" (*Glacial Geology of Great Britain*, p. 209).

Kendall says: "At the Leyland gravel pit (in Lancashire), so celebrated for its rich yield of shells, bored stones are said to be abundant, and I have myself found one there, but the workmen report them as far from common. A bored stone was found in the upper boulder clay at Newton, near Chester, fragments of the *Saxicava* which bored the crypts remained in the holes, and the holes were not filled with the red clay in which the stone was found but with sand" (*Quart. Journ. Geol. Soc.*, xxx., p. 181). "Another bored stone was found in the boulder clay at New Ferry. It was strongly planed and striated in the direction of its longer axis. . . . It was admitted that the stone might have been transported *after* all the boring was done" (*Nature*, xl., p. 246; Lewis, *Glacial Geology of Great Britain*, p. 427).

In a paper on the floor deposits of the Irish Sea which consist of drift, Messrs. Herdman and Lomas remark on the plentiful erratics of crystalline rocks encrusted with millipores and polyzoa, a fact showing they have not been rolled *in situ*. Striated stones and shells are not uncommon, especially where the bottom is muddy. "Apart from the shells glacially

striated, *are found many shells in a sub-fossil condition, strongly suggestive of glacial remanié.*" Again, speaking of the profusion of barnacle valves and pecten shells on the floor of the Irish Sea, they say "some of these have been derived from glacial *or even pre-glacial deposits*". In Mr. C. Reid's remarks on this paper he says: "Even in deep water (over fifty fathoms), stones measuring two or three inches in diameter may be found frequently angular, but sometimes well rounded. These cannot have been rounded by current action *in situ*, as they are found encrusted by delicate polyzoa in a fresh and unrolled condition. None of the stones seem to have been rolled at greater depths than twenty fathoms. In some deposits shells and stones are characterised by having received a high polish" (*op. cit.*, pp. 213, 217). All this is consistent only with the boulders and shells in question having been shaped and fashioned and altered before they were enclosed in the drift.

A propos of the boulders, we must not forget the so-called palæolithic implements occurring in the drifts, many of which show very plainly their derivative character and that they are not *in situ*. Their weathered and scratched surfaces, and especially the skin or patina with which they are covered, sometimes on both sides, sometimes on one only, as in numerous cases in the fine collection of Dr. Sturges, point, as he so emphatically argues, to their being much older than their present *provenance* would suggest. In a paper by Mr. George Clinch on the drift gravels at West Wickham, he says of some of the palæolithic implements he found that a large proportion of specimens have modified angles and a general appearance of smoothness and roundness. He attributes this to drift wear. In others he says there is no sign of such wear, but they retain their ridges and angles quite sharp. This seems to me clearly to point to the former class having been long exposed to weathering and other conditions, while the latter were either much newer or had been protected, and I see no occasion to invoke any glacial conditions to explain the phenomenon. I, however, completely agree with Mr. Clinch's concluding paragraph, where he says: "The association of much-worn implements, unworn implements and flakes, cores and waste chips in the same bed of drift

gravel points to the fact that we have here a collection of material which has been brought from a great variety of places and has undergone a great variety of conditions and changes" (*Quart. Journ. Geol. Soc.*, lvi., p. 9).

Mr. A. M. Bell, in his remarks on the paper, agreed with Mr. Clinch that these palæoliths had come from various sources and had travelled in some cases for great distances.

Dr. Sturges' very numerous specimens are most instructive in this behalf. Many of them which have come out of the gravels have one side quite black, the original colour of the flint not having been altered, while the opposite face is entirely coated with a milky white skin or patina, a condition of things only compatible with their having lain on the surface a long time before they were buried; others have the white patina covering them all over; and others, again, have apparently been rechipped round their edges after the patina has covered them. In addition to the patina, many of the palæoliths are marked by striæ, showing that they have been scratched by some force which has rubbed them against other stones before they were buried. All point, it seems to me, the same moral as the facts collected in this chapter as to the derivative character of the contents of the drift, and of its being composed of previously formed and shaped materials.

Sir William Dawson says that the beds at Isle Verte, Rivière du Loup, Murray Bay, Quebec, St. Nicholas, Little Metas, etc., often contain boulders and large stones covered with *Balanus Hameri* and with *Bryozoa*, evidencing that they have for some time quietly reposed in the sea-bottom before being buried in the clay. This is indeed the usual condition of the boulder clay in the lower part of the St. Lawrence river (*The Canadian Ice Age*, p. 38).

This evidence is all most instructive and consistent, and it shows that, so far as we can judge, the rounded and polished boulders we find in the drift beds are, like the organic remains found in them, derivative, and that they were ready-made when the force, whatever it was, that distributed them took them up and carried them along. In my view, they were for the most part home-made—that is to say, they were manufactured close by where their mother-beds lay, in all probability by the action of the sea, for they are quite unlike river gravels or

river shingle. I go further: just as we have seen that the greater part, if not all, the pebbles in the gravels and shingles of Southern England are rearranged tertiary pebbles, having come from Bagshot or Brocklesham or Reading beds, so do I hold these rounded boulders to be very largely of tertiary age.

Similar boulders, as we all know, occur in the crags of Suffolk. Thus Prestwich mentions finding a rounded boulder of dark red porphyry of considerable size and weighing about a quarter of a ton at the base of the coralline crag (*Quart. Journ. Geol. Soc.*, xxvii., p. 117). He says it is a question whence this block was derived, and he adds he knew nothing analogous to it in England or Scotland, and that it was still to be determined whether it came from Scandinavia or the Ardennes. The oolitic remains found in the same bed were probably derived from strata in Central England, while Prof. Dawkins referred a pliosaurian vertebra found there to the Oxford or Kimeridge clay (*ibid.*, p. 134). Elsewhere he speaks of the many fragments of secondary rocks and of mountain limestone in the red and Norwich crags (*ibid.*, p. 479).

The same kind of evidence is forthcoming in Switzerland. In the Alps no beds are more remarkable than the vast teriary conglomerates, whose disintegration it seems to me supplied the equally vast sheets of rounded boulders which cover with their stupendous mantles the lower grounds of Switzerland everywhere, and which are quite beyond the reach of any agencies now at work there or any conceivable ice action to fashion. For an explanation of these boulders we must go back to much earlier geological times, and to very different conditions.

Let us now turn to another feature of the so-called glaciated lands, namely, the rounded, *moutonnéed*, polished surfaces of primitive rock which occur so frequently in high latitudes, in most cases covered and disguised by great sheets of drift. These surfaces have been very generally pointed out as the crucial tests and evidences of the great ice age; and I do not know that anyone has ventured to ask whether they are not much older than they are generally supposed to be. It seems to me that this is an issue which ought to be raised and settled before so much is built upon it. I myself know

of no direct evidence for associating the date of the rounding of these surfaces with the period of the distribution of the drift, while there are some considerations which point in a very different direction.

In the first place, it is difficult, it seems to me, not to associate the polishing and rounding and eroding of these surfaces with the manufacture of the great mass of the crystalline rounded boulders and cobbles. They were apparently two results of one cause, and were made at the same time; but, as we have seen, the rounded boulders and cobbles are in all probability derivative, and belong not to the period when the drift was deposited, but to an earlier age, and probably to tertiary times. Hence it would follow that these smoothed and rounded surfaces are also older than the distribution of the drift, and probably also belong to tertiary times.

There cannot be a question that they are older than the drift beds actually overlying them. They cannot have been smoothed and rounded while protected from erosion by these deep and widespread cushions of soft materials. Some inquirers would make out that these soft beds are merely the moraine left by gradually and continually retreating ice; but, as we shall see presently, these mantles of sorted sand, gravel and boulder clay have little or nothing in common with moraines, either in regard to their contents, internal structure or external outline; and it seems impossible to doubt that the rounded and smoothed beds were rounded and smoothed before, and probably long before, the drift was distributed over them.

Again, if we examine the beds underlying the drift in many places where they do not consist of primary and crystalline rocks, but of softer secondary ones, we shall find their surfaces torn and gashed and weathered into deep holes and pits; and this is especially the case with the chalk, where the potholes are so often deep and ruinous and filled with tertiary sands, etc.—all proving that these surfaces were weathered and worn by a long continuous process before they were protected from further damage by being covered over with gravel, loam and sand.

Thirdly, in districts where we have the rounded and whaleback and polished structures most obviously displayed, we

often have these surfaces pierced by the so-called giants cauldrons—spherical deep holes with polished sides and lips, and clearly worn by the swirling motion of entangled boulders, which have been caught by some obstruction, and have then proceeded to turn round and round *in situ* like the millstones in a mill.

It has been the fashion to associate these giants' cauldrons with glacial operations, because they chiefly occur in crystalline rocks and they are in the main found in high latitudes; but they have nothing whatever to do with ice. Ice could not possibly be the agent by which the stones in the holes were turned round. This could only be water; and inasmuch as the polished surfaces of the rounded rocks are perfectly continuous with those of the cauldrons, we cannot well separate the agent which polished the one from the agent which polished and rounded the other; and it seems almost certain that in both cases we have to do with polishing caused by loaded water in motion and have nothing to do with glaciers. If a giants' cauldron could be made under a glacier at all, it must be by a glacier torrent which would be acting like any other torrent.

If we examine the situations where these giants' cauldrons occur on the tops of hills or on their slopes, as at Trollhätten, Lucerne, etc., we shall see further that it is not possible for water to have flowed over them in the shape of a torrential stream and set these boulders gyrating in the holes with the present contour of the country. That must have entirely changed since the cauldrons were made—which is another reason for dating them back beyond the time of the distribution of the drift.

Again, if we examine coasts in high latitudes which we have strong grounds for believing have recently risen from the sea, since the land there is terraced with old sea margins, we shall be very much struck with what we see. On the coasts of Norway and Southern Sweden we have a vast number of hump-shaped islands with smoothed backs, looking very much like a shoal of petrified whales. Pettersen, who knew the phenomenon well, refused to attribute it to glacial action at all, and argued, and, as I think, showed, that it was due to the action of the tide when loaded with stones, and

probably also with shore ice, working upon a shallow bottom. It is a feature of all rising coasts, and is especially noticeable in the islands off the coast of Greenland, and in those of the North American archipelago described by the various arctic explorers.

It seems to me that every analogy points to the similar phenomena in the various archipelagoes in the North Sea, the Faroes, Shetlands, Orkneys, Hebrides, etc., having had a similar origin, and this applies also to the rolling *moutonnéed* smoothed surfaces we find in Scotland and elsewhere.

Apart from this, which may be an arguable issue, we have certain kinds of evidence pointing to the time when this smoothing and rounding of the surfaces of the archipelagoes in question and of Scotland took place having been much older than is generally supposed. The animal and vegetable tenants of these archipelagoes cannot have reached them except overland, and speak, therefore, of a time when the greater part of the North Sea and of the Irish Channel was occupied by land. Inasmuch as the vegetation in question mantles and covers the rounded and smoothed surfaces in question, it shows that a great geographical change has occurred, causing a radical redistribution of land and water since these surfaces were smoothed and rounded as we find them.

On the other hand, the distribution of the shell beds *in situ* on the coasts of Scotland, which we have tried to show are older than the time of the distribution of the drift round the coasts of Great Britain, seems to show that the present coast lines have not materially altered in outline, nor has the level of the land materially changed since these molluscs were living. This seems to show that the separation of the islands from the mainland, and perhaps from each other, and the breaking down of the land bridge which united them, is much older than the distribution of the drift—*a fortiori*, therefore, must the smoothing and rounding of the whale-backed islands and the *moutonnéed* surfaces in question be older.

All these are cumulative arguments pointing to the surfaces in question being really relics and traces of tertiary erosion and denudation, and of their having originated, like the rounded boulders originated, long before the distribution of

the drift. I know of no evidence direct or indirect to set against or to qualify this conclusion.

A general and most important result from these facts and arguments is that, whatever it was that mixed and distributed the soft surface beds of the drift, it *had no part in manufacturing the ingredients out of which those beds were fashioned.* These ingredients were already fashioned and ready to its hands. The sands were already there in the shape of crag and Bagshot and Reading sands. The clays were there in the form of Chillesford clay, London clay, Oxford clay, Kimeridge clay, etc. The polished flint and quartzite pebbles were then in the form and shape we now find them. The chalky oolitic and liassic rubble was then in the form of rubble, and the far-transported crystalline boulders were rounded and ready for transportation; while the rounded, polished and *moutonnéed* and whale-backed rocks which attract us all wherever they occur *in situ*, and have converted many sceptics to the glacial faith, were there, and all, so far as we can see, belong to an older horizon altogether.

CHAPTER XIV.

STRIATED ROCKS AND BOULDERS AND THE ANGULAR DRIFT.

"It seems physically impossible that a sheet of ice could move over an uneven surface, striating it in directions uniform over vast areas and often different from the present inclinations of the surface. Glacier ice may move on very slight slopes, but it must follow these (since gravitation, along with the more or less plastic nature of the ice, has been shown to be the cause of its motion); and the only result of the immense accumulations of ice supposed would be to prevent motion altogether by the limit of slope or the counteraction of opposing slopes, or to induce a slight and irregular motion towards the margins, or outward from the more prominent protuberances" (Dawson, *The Canadian Ice Age*, p. 14).

One of the great stumbling-blocks attaching to the explanation of the drift which was current among "the Old Masters" was the difficulty of accounting for the enormous dynamical work necessitated by the facts and involved in the postulate of a great flood of waters to which they attributed not only the distribution of the drift but also the rounding of the pebbles in the gravel, the making of the boulders, and the rounding and polishing of the great rock surfaces. How all this could have been the result of any single or even any repeated flood of waters acting for a short time seemed incredible, and so it was and so it remains.

The same infirmity in another way attaches to the extravagant postulate of the ultra-glacialists. They also try to explain the whole of the phenomena in question by the operation of a great ice-sheet or of great ice-sheets which are supposed to have similarly manufactured the rounded pebbles in the gravels and the boulders, and to have smoothed and rounded the rock surfaces, and thus involve themselves in a demand not only for an ice period but for an immensely long one, a demand inconsistent with the facts.

I have argued and tried to show in the previous chapter

that it is a mistake to attribute the manufacture of the materials of the drift to any cause which acted *contemporaneously* with its distribution, and to show further that the pebbles and boulders and rounded rock surfaces, as well as the sands and clays and brick-earths associated with them, date from a much older time, and belong, in fact, to tertiary or even earlier days,—that in solving the drift problem what we have to find is not an instrument capable of producing all these effects, a mill capable of grinding all this geological grain, but only one capable of rearranging and redistributing materials which had been fashioned long before. In view of the evidence here collected, it seems to me incumbent on the champions of the glacial period to reconsider the problem they have so pertinaciously fought about, and to make a clear separation between two entirely separable and separate phenomena—the manufacture of the drift materials and the subsequent distribution of the same. This is a most material part of my case, and it seems to me that the evidence above collected puts the burden of proof of showing the contrary on the other side. In the previous chapters I have, in fact, tried to show that neither the organic nor the inorganic contents of the drift, with the exception of the angular boulders and the angular drift, are contemporary with the distribution of the drift. They are all derivative. They nowhere in themselves testify to any *prolonged drift period*, any more than the rounded pebbles, clay and sand distributed by a great inundation testify to that inundation having been a very prolonged affair. The evidence of a prolonged glacial *period* or drift *period* is not forthcoming. All we have evidence of is a redistribution and re-assortment of ready-made materials, which, for aught that we know, *primâ facie*, may have been a very rapid one. Before we turn to the mode of distribution of the drift and its explanation, we must, however, consider two matters which may be reasonably concluded to be largely contemporary with its distribution, and which stand in a different category to the phenomena described in the previous chapter.

The phenomena in question, which in my view have thus to be explained, are, first, the striation of the pebbles and the rock surfaces; secondly, the origin of the angular boulders and so-called angular drift. These are phenomena which seem

to me to be linked together as attributable more or less directly to the same cause, and as having occurred at the same time as the distribution of the drift itself. Were they the handiwork of ice or of water? That is the riddle which I have tried to answer a good many times, and am trying to again answer.

First in regard to the so-called glacial striæ. That these grooves and scratches which so wonderfully mark the surface of Scandinavia, Scotland, and other so-called glaciated countries were the handiwork of ice and ice only has become an article of absolute belief among all good geologists, and by good I mean all who conform to the Thirty-Nine Articles of the faith as laid down by the orthodox preachers of the science. It was not always so. Some of the keenest and wisest and most experienced among the older masters, such as Durocher and Sefstrom and others, utterly repudiated the contention, and it has certainly been by persistent, hard assertion rather than by forcible argument that the modern view has displaced the old one.

That a sharp-edged hard stone or a number of such stones will, when moved over a bed of rock with sufficient force and pressure, score and indent and scratch that bed is as certain as that a wood-engraver's tool will do the same on a wood block; and a cartoon in *Punch* is not a more certain witness to the handiwork of some wood engraver than is the polished and striated and scratched surface of Scandinavia of there having passed over it some powerful scouring and denuding force, armed with sharp stones or other graving tools. This is quite certain, and no one disputes it.

What Sefstrom and Durocher disputed, and what I venture to follow them in disputing, is the conclusion that this force must necessarily have been ice.

Ice, no doubt, when holding pebbles and stones in its grasp, does, in the instance of Alpine and other glaciers, sometimes scarify the bed and sides of the channel in which it moves. This I do not dispute. What I say is, that water when loaded with precisely the same stones will do the same thing; that the graving tools in either case would be the same. The only difference would be that in one instance they would be held in the grasp of a slowly moving viscous mass, and

in the other of a rapid moving but more fluid grasp. Any number of cases may be cited where flooding waters loaded with gravel have scored rock surfaces, etc., etc. The conditions of this work are very patent and obvious—namely, that the torrent shall flow very quickly and be loaded with more or less angular débris.

Here I would put in a *caveat*. We are continually reminded that there are only few scratches on river gravel and sea shingle; of course there are, because the stones in river gravel and sea shingle have had their edges rubbed down and are smoothed. A burnishing tool does not scratch a surface. In order to have scratches and groovings, you must have more or less angular and edged graving tools. With rounded and polished gravel, you may dig out troughs and hollows in a rock, but not scratch it with fine striæ.

I have in a previous chapter collected a considerable number of instances to show how striæ are continually being made on rocks and boulders by very ordinary processes far removed from ice-sheets (*ante*, vol. i., pp. 173-178). To these I may add a few more. First in regard to scratched boulders. Nothing is so attractive to a glacialist as a scratched stone, and to many glacialists a scratch on a stone means a former glacier. Let us test this.

In the report of the *British Association* for 1880, p. 386, Sollas exhibited a striated fragment of carboniferous limestone from the triassic breccia of Portishead. The striation, he says, was not due to glacial action, but is of the nature of a slickenside. The fragment was derived from the neighbourhood of a great fault. It serves, he says, as a caution in receiving statements with regard to the finding of striated fragments which had not been submitted to competent authorities. The same geologist has described a number of triassic pebbles from Polskemet, Monmouthshire. They were of all sizes up to a foot or more in diameter, and well rounded at the edges and corners. The smoothed surface of these pebbles is abundantly striated, especially on and around the edges and corners; the striæ commence as exceedingly delicate fine lines, which frequently deepen and widen in their course till they terminate abruptly so as to present the form of a half-cone. At the deep end of the trough (base of the cone)

a grain of quartz sand is sometimes found imbedded. Sometimes, however, the striæ are mere scratches, thinning out at each end; they are not always straight, but sometimes curved, a whole group of parallel striæ being occasionally abruptly flexed to one side and then back again into their original direction. Now and then a furrow as much as a quarter of an inch wide may be observed, its sides being scored with delicate parallel striæ. Mr. Sollas then goes on to explain how these striæ were produced, namely, "by the presence of sand grains in the matrix about the pebbles, which, when the pebbles were pressed together, thus exerted pressure on the sand grains lying between them." That there has been such pressure, he says, is shown by the fact that some of the smaller pebbles are sinking some distance into the larger as though they had been pressed into a yielding substance. This pressure he derives from the thick beds of sediment which once covered the conglomerate, and which would be resolved not only in directions normal to the touching surfaces of the pebbles, but also in others leading to movements along lines of least resistance. The quartz grains lying between the pebbles would not only be pressed against but dragged over them. As they began to move they would produce a delicate almost imperceptible striation, and with continued progress this would deepen, the grit would "bite" more deeply into the stone, and would at length become too far embedded to overcome the resistance in front; then it would be brought to a standstill, and remain as we now find it, implanted at the end of the trough which it has excavated. He goes on to suggest that "the pebbles and boulders formed the beach of the triassic sea, that under great pressure they were packed closer together, forced one into the other and pitted all over with embedded sand grains; lateral movements dragged along the sand grains over the surfaces of the pebbles, scoring them with delicate furrows and striæ. Subsequently the dolomitic parts between the pebbles cemented into a hard matrix, and bound the whole together into a conglomerate." This is a most instructive diagnosis, and one which should be taken to heart by those who fancy that every scratched stone or every heap of scratched stones necessarily bespeaks a glacial origin. Mr.

Sollas says *emphatically*, "glacial action is here indeed entirely out of the question" (*Geol. Mag.*, 1881, pp. 79, 80).

Speaking of the neighbourhood of Aglish, in Ireland, Carvell Lewis says no true glacial drift is observed. Drift consisting of a mixture of sharp and rounded fragments, some of them scratched, occurs on the sides of the hills. I take it to be "creep drift" or "avalanche drift". The scratching of the fragments is due to sliding; all the material is local. The soil is a mixture of sand and clay; the fragments have their flat sides down (*Glacial Geology*, p. 135).

Speaking of Haulbowline Island in Cork Harbour, Lewis also says: "At the lower end of the island, where there is much 'made ground,' there is an impure clay, full of stones, shells and boulders. The latter are often scratched. None of this is more than five feet above the sea. The scratched stones were scratched perhaps artificially. At least they were not scratched by a glacier. There is another case of scratched stones occurring in clay outside of the glaciated area. They probably came from below the sea-level.

"A similar case occurs in the Severn, where during the excavation of a tunnel under the bay scratched stones were found, as Prof. Sollas has informed me. I expressed the opinion in Report Z. that scratched stones are characteristic of a glaciated area, but I have changed this opinion, having seen scratched stones at many places in America, Ireland and England outside of the region covered by land-ice. Scratched stones abound all over the South of England, near London, Birmingham, etc. I found them on the Susquehanna, coast of Berwick, at Oil City, Clean Rock City, etc., outside of the glaciated area; and in Ireland about Listowel, etc. I found them also in the stratified drift south of Dublin" (*ibid.*, p. 140).

Speaking of Pembrokeshire Dr. Hicks says: "I may here give a note of warning to those who are likely to visit the district not to be deceived by the parallel markings which are frequently seen on the boulders, and which, after a certain amount of weathering, look exceedingly like ice scratches. . . . The more conspicuous markings have been caused by the passage of a plough or harrow across them when lying just

below the surface of the ground" (*Glacialists' Magazine*, 1894, p. 6).

I do not dispute that glaciers sometimes striate boulders. It is quite true that some of the stones found in moraines at the bottom of living glaciers are striated, but their number is comparatively very small compared with those which can be found in Alpine valleys as the result of land slides. But whether the number be small or great, it is clear that in modern moraines striated boulders do sometimes occur. It does not follow, however, that ice has had anything to do with many or most of them. A moraine is a medley and mixture of boulders, sand and clay. The boulders are of two kinds, angular and worn. The worn boulders again are of two kinds; a large proportion of them are rounded and have had all their angles smoothed down by a clear process of *rolling*, and this, if the boulders are not from older beds but made on the spot, could only have happened in sub-glacial streams and not as glacier-held stones. We cannot understand how the latter could roll at all, or if they could, their motion in turning would be so infinitely slight and slow that we cannot understand it producing boulders or marks on them. True ice stones have parallel sides, or one side smoothed down by being held down by the ice against the glacier bed, sometimes with a slight layer of sand in between, and they have thus got their smoothed faces slightly striated. Such stones no doubt occur in true moraines. They are a small fraction of the whole. The larger fraction being true water-worn and water-rolled boulders.

I should be content with the modes of argument of the glacial champions on this point if they were to put out of the boulder clays these stones with smoothed and parallel faces showing striæ, and limit their conclusion to them. Their case would indeed be a small one if this were their method of argument. On the contrary, with them every striated stone is an ice stone, whether it be a kidney boulder or a boulder with rounded angles, *i.e.*, plainly water-rolled stones or so-called slipper stones or skids.

A most important document in regard to the question we are discussing is Colonel Fielden's paper on the "Glacial Geology of Arctic Europe and its Islands" (*Quart. Journ. Geol.*

Soc., pp. 611-652, etc.). Colonel Fielden found on the island of Kolguef, situated between Nova Zembla and the mainland of Russia, a vast number of striated boulders, and Dr. Hinde, in the discussion which followed the reading of his paper, found them so typically the kind of striated boulders upon which glacialists have built up their theory that he said: "The distinctly striated boulders and the other specimens brought by the author from Kolguef *indicated that the clays in which they were embedded were of the nature of genuine boulder clay formed beneath a glacier*". Colonel Fielden, whose experiences of Arctic geology few, if any, could match, was most emphatic on the other side. He said he could assure the Fellows *that the Kolguef beds are as certainly sedimentary beds as the Thanet sands or the thalassic ooze of Barbadoes*, and he consequently entirely disputed the view propounded by Dr. Hinde and Dr. Gregory that the deposit was that of an ice-sheet.

In regard to the boulders in question, he says that "all the streams in the island in the latter part of their course run over stones and boulders which have been washed out of the surrounding mud beds. An examination of these stones, which are of every shape and form, from angular fragments to rounded and polished rocks, *shows that a large proportion are ice-scratched*" (say rather scratched, H.H.H.). "The medley of rocks represented," he says, "is remarkable, granites and gneisses, limestones, silurian and carboniferous grits, quartzites, porphyries, a variety indeed so great that it would take a trained petrologist to enumerate them," and he describes them as "lying in the mud in most disordered fashion like currants in a cut-loaf. One that lay in a stream bed a few miles north of a river was a huge block of very hard yellow sandstone, polished, scored and striated. Along its major axis it had deep flutings cut into it, and in addition it was transversely scratched. It measured fifteen feet in length, nine feet in breadth and six feet in height." Colonel Fielden adds: "I have no altogether satisfactory explanation to advance for the presence of such immense numbers of ice-scratched stones as occur in these sedimentary beds of Kolguef. The action of an ice-sheet cannot be invoked at Kolguef; yet seeing that the ice-scratched (say scratched, H.H.H.) stones throughout the Kolguef beds must in the aggregate amount to millions, one

naturally asks where were they manufactured? Undoubtedly stones frozen into the bottom of floating ice become scratched and polished when stranded on shores where there is sufficient rise and fall to admit of the ice-rafts grating. I have seen this process of manufacture going on. It is, however, difficult to conceive a train of circumstances which admitted of erratics being transported in floe-ice to some rocky coast, there to be scratched and then carried out to sea, and deposited on the floor of the ocean. . . . If we admit that the Kolguef erratics came from glaciers, where do we suppose those glaciers to have existed during the period when the Kolguef beds were deposited?" This is all surely very eloquent, especially if we accept, as I do, my friend Prof. Bonney's conclusion that "we ought to explain British deposits by those of Arctic regions rather than to follow the reverse process" (*ibid.*, p. 65). Colonel Fielden's concluding words must have a very harsh and troublous sound to the champions of ice-sheets and the *Glacial Nightmare.* He says: "It is suggestive that all the glacial deposits which I have met with in Arctic and Polar lands, with the exception of terminal moraines, now forming above sea-level in areas so widely separated as Smith's Sound, Grinnell Land, Northern Greenland, Spitzbergen, Novaya Zemblaya and Arctic Norway, should *be glacio-marine beds.* Throughout this broad expanse of the Arctic regions I have come across no beds that could be satisfactorily assigned to the direct action of land-ice; that is to say, beds formed *in situ* by the grinding force and pressure of an ice-sheet. On the contrary, so far as I can judge, the glacial beds which I have traced over the extensive area mentioned above have all been deposited subaqueously and re-elevated" (*ibid.*, pp. 57, 58).

Let us now turn from striated boulders to striated surfaces. Here again I have previously collected a number of instances to show how frequently such striations were due to much more prosaic causes than ice-sheets (*ante*, pp. 173-178). I will quote a few more.

The occurrence of such striæ on crystalline rocks in the very hottest parts of India, as for instance in Scinde, seems to be almost conclusive evidence that they are due in many cases to other causes than ice action. Thus Blandford describes how in one place the surface of the porphyry underlying a

boulder bed was unusually smooth and distinctly striated, the striæ running north-east and south-west (see Records, *Geol. Surv. India*, x., p. 14, etc.).

Sir W. C. Trevyham describes on the way from Megara to Corinth the road as passing along the base of the cliffs where the limestone bed was nearly vertical for about 200 feet in length and about fifty in height. Wherever it is protected from the weather it is highly polished and scratched, several of the scratches extending for several feet, so as to be nearly parallel with each other and vertical. When they are not water-worn he compares them with those on the polished limestone of the Jura and Neuchâtel, which they also resemble in texture and colour. Not having detected traces of glacial action at much higher elevations on Mount Parnassus, he considered that in this latitude and at such a low level the scratches could not be attributed to glacial action or floating ice. Having found a portion of rock apparently in its original situation in contact with the polished surface, he was led to conclude that this was a case of "slickensides," perhaps the effect of an earthquake; and that the scratches may have been produced by particles of sand or chert between the two surfaces when they were put in motion. He noticed at the opening of a gorge on the south-east flank of Parnassus, above the town of Daulia, extensive mounds of gravel, débris and boulders derived from the upper end of the gorge, and resembling in form both longitudinal and transverse moraines, and including small lakes or pools, but as there was no evidence of glaciers, he looked upon the phenomena as the result of floods caused by melting snow and avalanches and storms (*Proc. Geol. Soc.*, iv., p. 203).

The same kind of witness is borne by the occurrence of such flutings and striæ in the older beds whose structure and contents prove them to have had a tropical or semi-tropical origin and remove them from all possibility of ice action thus.

In a paper on groovings and boulders found in the Dukinfield coal mine, Mr. J. Radcliffe, F.G.S., says: "The single groovings vary in depth from a few inches to eighteen inches, and in width from one foot to three feet. Occasionally several of these grooves run close together and then widen out into one broad 'scour' or groove of fifteen or even

eighteen feet wide, as if the base of a large moving body had come in contact with the upper surface of the coal bed. These grooves have been traced at intervals over a distance of from 500 to 600 yards in length, and from fifty to sixty yards in width. The mean line of bearing of the grooves is south to east. This line is not connected nor nearly parallel with any fault or fault slips, and there are no dislocations where the grooves occur. The grooves run down into the coal, and are filled with shale like that on each side of the coal."

Mr. Blandford in the discussion suggested that the grooves might have been caused by floods at the time when the beds were being deposited. He had seen such excavations in plains over which water had passed (*Quart. Jour. Geol. Soc.*, xliii., pp. 509-604).

Let us, however, come nearer home. In regard to the scratched and polished surfaces near Edinburgh attributed by Buckland to ice and by Sir James Hall to a flood of water carrying detritus with it, Murchison, who visited the place in 1840 with Buckland, Graham, Maclaren and others, was satisfied they were simply the result of the changes which the mass of the rock underwent when it passed from its former molten or pasty condition into a solid state. "These appearances differ essentially from ordinary glacial scratches or scorings. (Plaster casts of them exist at the Geological Society.) They are in fact broad undulations or furrows, and instead of trending *from* the higher ground to the Firth of Forth, as would naturally be the case if they were due to the expansion and descent of glaciers, they rise up to the very *summit* of the low ridge in a direction transverse to its bearing, and with no neighbouring point of ground higher than that on which they occur. On clearing away the thin turf which barely covered the rock, some of these undulations appeared wide enough to contain the body of a man, and though observing a wide sort of parallelism their forms were often devious. As their surface was smooth, not much unlike the usual aspect of the so-called *moutonnée* rocks, the glacialists of our party at first seemed to be proving their case, when suddenly a discovery destroyed, at least in my opinion, their theory; for in the adjacent quarries of

the same hill, at a much lower level, and upon beds just uncovered by the workmen from beneath much solid stone, other sets of undulations or grooves were detected, so like to those on the summit of the hill, that a little atmospheric influence alone was required to complete their identity. My belief, therefore, is that the undulations were caused by the action which took place when the stone was solidified" (Murchison, *Geol. Proc.*, iii., pp. 675-676).

Mackintosh, a keen-witted and experienced geologist, sharply criticised some of the evidence in regard to fluted rocks in North Wales. He first takes a case cited by Buckland below the bridge Pont Aber Glasslyn, near Beddgelert, on the right bank of the river. Although most of the striæ on the polished rocks there were generally parallel to the river, others were oblique and some opposed to its direction; the flutings there he also thought were more regular, more accurately parallel and more symmetrically placed than could be the case if they had been produced by the passage of a glacier. The flutings in Snowdonia are uniformly and strictly parallel, and do not vary in depth or section; they are at equal distances apart, and are very similar in different valleys, and he attributes them to a structural origin, as he also does more or less oblique and irregular striæ.

In the valley of the Llugvig, between Pont-y-Gyffing and Capel Curig, described by Buckland, some of the flutings on the Bangor side of the great dome are opposed in direction to the valley, although agreeing in every respect with those which are in its direction, and he holds them to be natural furrows. On some low rounded rocks about 200 yards from the dome-shaped masses on the western side there are furrows at right angles to one another. The same thing occurs on a small mass of schist about midway on the north side of the Lower Llanberis Lake, and yet these rocks are regularly furrowed in continuous parallel lines, not only from top to bottom, but on each of the sides accessible to view.

Secondly, the parallel marks on the conglomerates opposite Capel Curig are broader, deeper and wider apart than on the underlying schists, agreeing apparently with their wider and larger lines of bedding.

Thirdly, in these conglomerates are fragments of schist,

some of them half a foot square, marked with regular parallel flutings at equal distances, exactly in the same manner as the rocks *in situ;* the matrix is free from such marks, and they are not in any definite direction, but depend on the position of the fragment in the conglomerate; the buried and lower faces of the fragments in the conglomerates are fluted like their exposed faces. These flutings must have been made before the conglomerate was formed. At one place on the right bank of the Lower Llanberis Lake, the flutings on the coarse blue slate cease when the conglomerate begins.

Mackintosh treats the striæ not parallel to one another as structural, since they occur in narrow recesses of rock, the edges of which are angular, and on the projecting fragments of beds, the rest of which have been denuded. These striæ are generally narrow and shallow lines, seldom more than a quarter of an inch in breadth, but usually less, and some of the thinner ones have the appearance of straight scratches, and seem to be parts of weathered cleavage lines appearing interruptedly here and there. Other striæ more or less oblique and irregular, and occasionally intersecting the former, he thinks, have arisen from cracks. These last are rarely curved, thus differing from the glacial striæ described by Agassiz, and are often bent at an angle, and often so as to make it difficult to see how a descending body could make them. Many also have flat surfaces different to those made by angular fragments; they much resemble cracks occurring on schists as seen in the Penrhyn quarries and on the Ogwyn Lake.

In regard to the flutings they are always parallel to the cleavage, and correspond to the cuts or series of narrow lines or joints called by the quarrymen "water-splits," which are also parallel to the cleavage and about half an inch apart. These water-splits are sometimes quite open, sometimes partially filled up, and in others the process is further advanced and the furrowed appearance is distinct. In width they also greatly resemble the flutings, and he concludes that the "water-splits" or "open cleavage joints" have been the origin of the parallel flutings and open cracks of most of the striæ. The weathered coatings of schists have often striæ on them, which though requiring a magnifying glass to see them show clearly as minute delicate lines of an inch or two in

length, very numerous, more or less straight and often intersecting one another. They are resemblances in miniature of apparent scratches on the rocks. He attributes to structural causes the markings which in America have been attributed by Hitchcock to glacial causes.

It is no uncommon thing in Iceland, says Watts, for huge masses of glaciers to slide down the mountain sides during periods of eruption, scratching the harder and furrowing the softer rocks in their progress, and leaving heaps of débris in no way distinguishable from terminal moraines. These facts are rather startling (Watts, *Across the Vatna Jokull*, p. 191).

I will lastly quote some criticisms of a very ardent glacialist, *viz.*, C. Lewis.

Speaking of the marks on the granite north of Mount Sorrel, Lewis says: "They closely resemble glacial striæ, and were held to be such by Mr. Harrison and others. A careful examination has convinced me that this is not an example of true glacial striæ, but is a very local smoothing due to moving gravel, or possibly an iceberg, and Prof. Bonney and Prof. Hill have come to the same conclusion. The whole exposure is only two or three yards long, and the rest of the region shows no signs of glaciation. Another locality for striæ of a similar kind is reported from near Nottingham (see *Quart. Journ. Geol. Soc.*, 1886, p. 460). There, under a heavy bank of boulder clay, striæ were seen on limestone. The locality is now inaccessible, and is probably an instance of iceberg striæ. Mr. Hart writes to me of another instance on the soft oolite south of Lincoln. Some of them may be due to the post-glacial and gradual slipping down hill of a mass of boulder clay, coming under the designation of what I have called 'creep striæ' in Pennsylvania" (Lewis, *Glacial Geology*, etc., p. 61). On creep striæ see Report Z., pp. 35, 96.

Lewis again speaks of re-examining the supposed glacial markings preserved in Queen's College, Cork. They are on limestone from Farranmacteige, Jennings Quarry. "The limestone was smoothed off and marked, not by long parallel striæ like glacial striæ, but by more irregular markings. . . . It seemed as if the limestone were smoothed simply by the

water and pebbles which were washed over it. The striæ also were made by the moving pebbles, the limestone being quite soft and easily scratched. It is very interesting," he adds, "to see how closely true glacial striation can thus be imitated. Prof. Sullivan of Queen's College informed me that these markings were only seen in one or two places, very locally. The elevation of both these localities was less than seventy-three feet above the sea. A drive over the adjacent hills of red sandstone and about Cork showed no traces of glaciated surfaces or of till" (*Glacial Geology*, p. 107).

The supposed glacial striations which Dr. Crosskey has described on the trap of Rowley Regis are, says Carvell Lewis (so far as I could see), due either to plant roots or to ploughshares, being totally unlike true glacial striæ, but identical with similar superficial markings I have seen upon trap rocks in the non-glaciated part of America (Lewis, *Glacial Geology*, p. 64).

In regard to this basalt at Rowley Regis, near Wolverhampton, the same writer again says he examined the supposed glacial striæ. "These marks or furrows," he says, "are only on decomposed rock, and on rock fragments near the surface. They are often curved, and usually start from an edge, being on angular faces like ogham stones. Furrows cross one another. They are probably due to tree roots, or perhaps to the action of humic acid on the rock. No trace of drift or glaciation is seen. A local creep down hill brings angular fragments of the basalt down on the top of 'the Rotch' (*i.e.*, the decomposed basalt). At the Rowley Regis station is an interesting grey sandstone, made out of the basalt by decomposition, undecomposed pieces of basalt remaining in it. Had glaciers or floods been over here it would have been removed by them. A proof that the furrows in the basalt were made by glaciers is said to be the fact (?) that they were made before the rock was decomposed. Since the decomposition layer was equally thick under the scratches as elsewhere, I find just the opposite to be the case, and that the furrows were made mainly in the crust, which was thinner under the scratches" (*Glacial Geology*, p. 301).

About similar markings on trap in America see Lewis, *Proc. Amer. Phil. Soc.*, on the great trap dyke across Pennsylvania; Pengelly, *Trans. Dev. Ass.*, vii., ix., xii., who describes cross markings on trap at Conglebourne, near Totnes; and Somervail, *Trans. Edin. Geol. Soc.*, v., pt. 1, p. 87, who thinks the above are due to a plough.

Lewis thinks the smoothing of the granite in Charnwood Forest was due to the passage over it of *water filled with gravel*. The traces of striæ, he says, could thus easily be made. I suspect that the striæ on the limestone at the Stanton Tunnel (*Quart. Journ. Geol. Soc.*, 1886, p. 461) are due to the same cause.

Lewis quotes again from Ramsay (*Quart. Journ. Geol. Soc.*, 1862, p. 1862), who says that floating ice can striate and polish rocky banks. Thus he says: "Marks of glaciation lie low down on the sides of the Moselle, where the floating ice has frequently rounded, polished and striated the rocky banks in the direction of the flow". But, as he adds, "there are no signs of glacial drift in the valley of the Moselle, or in the Eifel, or about Bonn". Sir Wm. Logan says "that ice in Canada charged with boulders has been known to produce grooves on the face of cliffs as well marked as those of glacial times (*Quart. Journ. Geol. Soc.*, 1872, p. 395; see also Forschhammer in Lyell's *Principles*, pp. 231-232; Bermot, *Z. D. G. G.*, 1879, xxxi., p. 145; and Lewis, *Glacial Geology*, p. 321).

These instances are useful because they are concessions from the prophets of ultra-glacialism. They show how very unsafe is the deduction generally made by the glacialists, who, whenever they see a scratch on a stone, fancy they are justified in summoning an ice age to explain it. What they unmistakably prove is that other causes than moving ice or ice-sheets can cause groovings and striations on rock faces as on boulders. The fact is, such scratches can be made and are made whenever a sharp-edged hard material is pressed against another while in motion, and the ice in a glacier is only one out of many methods in which stones may be pressed against other stones when in motion. To attribute such striæ wherever found, therefore, to ice action seems to be quite inconsequent. In those parts of high mountain valleys which are known to have been

recently occupied by glaciers in motion, we are justified in extending the induction to parts of the valleys from which the shrinking glaciers have departed, but to go entirely beyond this kind of evidence and to explain in this fashion the local striæ on rock surfaces occurring far away from mountain valleys such as those in the forest of Fontainebleau, or on the rocks underlying the drift in North Germany and round about Charnwood Forest, scores of miles, in some cases, away from great mountains, is to leave induction far behind, and to forget that whatever force dragged this drift over the surface, as it must have been dragged, under any hypothesis, must have scratched and worried the rock surfaces over which it travelled. To attribute such scratchings to ice is to impose a stupendous burden upon our credulity, and to invoke a steam-hammer to crack a nut.

A still more transcendental appeal is that which tries to explain the striæ, and may, I add, also the smoothed surfaces on high mountain *tops* by the operation of ice. Let me quote an example. Dr. James Geikie says: "Scratched and polished rock surfaces are by no means confined to till-covered districts. They are met with everywhere and at all levels throughout the country, from the sea coast up to near the tops of some of our higher mountains. The lower hill ranges, such as the Sidlaws, the Ochils, the Pentlands, the Kilbarchan and Paisley Hills, the Cheviots and others *exhibit polished and smoothed rock faces on their very crests.* Similar markings streak and score the rocks up to a great height in the deep valleys of the Highlands and Southern Uplands, and throughout the Inner and Outer Hebrides and Orkney and Shetland the same phenomena constantly recur. The direction of the parallel ruts and striations coincides, as a rule, with the line of the principal valleys. In the Northern Highlands, for example, they keep parallel to the trend of the great glens, and in the Southern Uplands, likewise, they follow all the windings of the chief dales and 'hopes'" (*Great Ice Age*, pp. 66, 67).

It will be seen that Dr. Geikie is here talking of the scratching and polishing of the very crests of the hills and the cols in districts where, as he describes, the striæ follow the valleys and radiate from their culminating heights.

That is to say, he is speaking not of ice-sheets moving quite irrespectively of the drainage of the country, but of true glaciers following the divergent valleys that radiate from great mountain centres. If we examine modern and living examples of such glaciers, we shall find that the polishing and scoring of the flanks and the floors of the valleys in which they move is not shared by the upper parts and the crests of the mountains; and this for two reasons, one being that the soft snow and *névé* which form the glaciers when nurslings are not capable of such dynamical work; and secondly, that for the most part the crests of the high mountains have got no deep masses of frozen material upon them; these latter really accumulating in the reservoirs formed by the valleys on their flanks.

Let us now turn to the conditions of the postulated glacial period. In that period of supposed extreme cold the snow-line would be lowered enormously. This is conceded by all the champions of the glacial age known to me. This lowering of the snow-line means that the amount of rainfall as distinguished from snowfall would be enormously decreased, and conditions very like, in some respects, to those of the Tibetan flanks of the Himalayas and the higher Andes would prevail, namely, that instead of large developments of ice the snow which fell would remain dry snow. It is overlooked by the glacial champions that snow cannot be converted into blue ice without considerable moisture and rain as well as pressure. These are concurrent conditions of such conversion. Hence why it has always seemed to me very difficult to explain how there could be the great mass of ice which is generally demanded from an ice age. Such an age would be more accurately described as a snow age. This is a general argument, however. Let us rather consider the particular case before us. Whether there was ice on the plains in the glacial age or not, it seems to me perfectly inevitable that with an immensely lowered snow-line there could be no ice on the high mountains and certainly not on their crests and summits. Their crests and summits would only be carpeted or blanketed by soft snow, which is a matrix quite incapable of holding a graving tool in its grasp.

How could stones be held by snow so tightly that they

would be pressed against the walls and faces and floors of the rock and leave their spoor in striæ such as we have been describing? The thing seems absurd.

Not only so, but how could the necessary stones be obtained for doing the work. As the very crests of the mountains are grooved and striated, it follows that they must have been buried under snow; and if it was snow and ice that impelled the graving tools there must have been perpetual snow in such positions in glacial times; that is to say, the summits would always be covered. There would be no rocks overhead, therefore, from which fragments of stone could be detached to act as graving tools, while the uniform conditions of temperature caused by the snow blanket would prevent the splitting and breaking of submerged rocks from differences of temperature. Even Prof. Chamberlin, who believes that glaciers can pluck stones out of their own beds, would hardly attribute this capacity to soft snow. From whichever side this question is faced critically, therefore, it melts away into absurdity.

Let us now turn to another similar problem. We have been considering the case of the magnified and glorified ancient glaciers to which some glacialists turn for help. Let us now turn to the postulated ice-sheets which we are so often told were quite different in every way to glaciers.

One of the peculiarities of the striæ which have been attributed to the action of ice in the ice age is that they are not in many cases divergent and radiating, but run for miles on more or less straight lines. In America they are said to run for as many as twenty miles right across the country irrespective of its contour, and over hill and dale keeping to parallel lines. Thus, to quote some examples, Dr. Geikie, speaking of the striæ in the Lowlands of Scotland, says: "Their direction does not appear to be influenced so much by the configuration of the ground, for they often cross low valleys at right angles, or nearly so, and sweep up and over intervening hills, even when these happen to have an elevation of more than 1,800 feet above the sea" (*Great Ice Age*, p. 67). Similarly De Rance told C. Lewis that on the north side of the Lake District striæ certainly cover the highest mountains 3,600 feet and run across them (Lewis, *op. cit.*,

p. 371). Sir Wm. Dawson (*Canadian Ice Age*, p. 42) says that in New England striation is said to have been observed on hills 4,800 feet high, as for example on Mansfield Mountain, where, according to Hitchcock, there are striæ bearing south 30° east at an elevation of 4,848 feet. These quotations might be greatly multiplied. The question for us is how is it possible to attribute these striæ to the action of ice.

In the first place, we are told by its champions that the ice to which the phenomenon is attributed did not work in the form of glaciers, but in that of an ice-sheet, and it could therefore move over the country irrespective altogether of the contour of the surface. Now it must be remembered that precisely the same infirmity in regard to the manufacture of blue ice from snow in so-called glacial times attaches to an ice-sheet that would attach to the deposits on high mountains to which I have already referred. With the snow-line greatly lowered (possibly lowered, it has been suggested, in our latitudes to the sea-level), it is exceedingly difficult to know how snow could be converted into ice at all, and until so converted it would seem that there would be neither viscous flow in it at all, nor yet a capacity for holding graving tools. This seems to me to be a stupendous difficulty, but let it pass.

Suppose we get our ice, our initial difficulty is not ended. So far as we can judge from laboratory experiments, the amount of viscosity in ice depends very largely upon its temperature, and it is most viscous when nearest the melting-point. It seems a necessary consequence of the hypothesis of an ice age that the general temperature must have then been very low, and consequently that the viscosity of the ice must have been very slight. This must have prevented even the slight and scarcely appreciable movement which is now found in the nether layers of a glacier (which alone hold the necessary tools for making the striæ), and the movement of these nether layers must have been reduced to *nil*.

If so, how is it possible to understand how the stones, which are supposed to have acted as gravers, could be not only pressed against the bed of the ice-sheet, but also score and cut out fissures and striæ. This not merely on level ground, but when the ice is supposed to have travelled against the resistance of the rock and against the stupendous pressure

of the ice mass itself, and this not for a few inches or feet or yards but for many miles, not in devious lines but in straight ones.

The position seems to me absolutely fantastic when thus analysed. This is not all, however. It is quite plain that if we are to explain the phenomena of the drift by means of ice sheets, we cannot postulate that they always or even generally culminated on the highest ground, but the reverse. The culmination must in certain notable cases have been on the lower ground. Take, for instance, the case of Scandinavia. There the striæ, the long striæ, march right athwart the Dovrefelds. This is also the route of the erratic stones which occur in the western parts of maritime Norway. These travelled stones and striæ, it is reasonable to suppose, are closely connected. The former have been traced almost entirely to the low country of Dalecarlia and Central Sweden; to the east of the mountains in fact, whence they have travelled right over the chain westwards. It was not only westwards that they moved from Central Sweden, however, but also in a much greater number southwards. This journey of the ice southwards one can perhaps make a mental picture of, even when our mind and judgment completely revolts against the possibility of their journeying right across the Baltic and for 600 miles over the flat plains of Poland. Such a journey would only, however, involve travelling over a flat or slightly undulating country, where the stones would not be travelling across the gradients of the land to such an extraordinary extent. But what are we to say about the movement of the ice westward, which is supposed to have caused the striæ on the Dovrefelds?

How are we to understand any substance endowed with either liquid or viscous flow deliberately turning aside from the path of least resistance and least friction, which was open to it, and setting off on a climbing expedition over great mountain chains, and facing every difficulty of gravity, and scoring and striating the rocks on its way? This extraordinary mechanical property of ice may be put beside the similar property of water, to which I have referred in earlier pages, which is supposed to have given it a taste and capacity for cutting itself channels at right angles to its flow, through crystalline ridges of mountains, when it had perfectly free and open channels to flow away by. The notion can only

be described as preposterous. It must be remembered also that the ice in question had so little *vis a tergo* in it that, as Pettersen has shown, it could not carry its burden of stones as far as the line of small islands along the Norwegian coast. This instance from Norway might be matched in many other places. The notion of the ultra-glacialists seems to be that ice when flowing deliberately prefers to travel up hill rather than down hill.

This is not all, however. Since the stones that are supposed to have made the scratches came from the central parts of Sweden and travelled thence sporadically, we have to postulate that the great ice-mound or ice-sheet to which their movement is attributed culminated there. Why should it culminate there? Why should an area, now remarkable for its dryness, have developed a special aptitude for accumulating frozen moisture unless we entirely alter the physical contour of the country, which the glacialists do not profess to wish to do? I have never seen any suggested answer to this query.

Again, inasmuch as the ice-mound which carried the stones is supposed to have taken them right over the Dovrefelds and used them to make the striæ which cross that chain more or less at right angles to the lines of least resistance, the position necessitates that it must have been much higher than the Dovrefelds themselves. This is the only way in which the mechanical problem of travelling over them by the viscous flow of the ice is explainable; but if this was so, how is it that the Dovrefelds in their very highest parts are free from ice action and show no signs of smoothing and plaining down, but are ragged and weathered? The position seems inconsequent.

If, again, the mound of ice was of the portentous dimensions required by the hypothesis, it must have completely covered up the whole of the low ground from which the stones were derived under thousands of feet thick of ice and snow; and if so, there would be no stones available for graving tools, for these, so far as we know, come from the so-called *nunatakker* and other crests projecting above the ice as in Greenland, where, when we have no *nunatakker*, we have no stones carried by the ice-sheet.

We shall be told, perhaps, that we can fall back in such a case upon the notion already criticised in previous pages, and patronised by Chamberlin and others, which seems to me to be entirely contrary to every physical probability, namely, that an ice-sheet can by its mere pressure crush its crystalline rocky bed into fragments, and can then proceed to pluck these fragments up by the roots, like a dentist drawing teeth, although sitting on them meanwhile with its portentous weight.

Both processes are equally incomprehensible to me. Surely long before the pressure was sufficient to break up a floor of crystalline rock the column of ice, which is so much softer, would itself be crushed and reduced to mere slush. When we plant a heavy cathedral on a rocky foundation, and there happens to be a slight differential strain on parts of the building, it is not the beds of rock below the foundations which are broken up, but the weaker pillars and piers and walls of the cathedral which give way; *a fortiori* would this be the case with a substance like ice. Chamberlin's notion involves, it seems to me, trying to break a great mug of stoneware by thrusting one's hand into the dough which it may contain. Still more incredible is it that, with a pressure sufficient to crush its bed pressing down on every square inch of that bed, pieces of stone should be plucked up out of it by the ice. I don't know how such a process could either start or continue. Messrs. Maskelyne and Cook profess to put a fat alderman in a chair, and then make him raise his own chair with his own body in it into mid-air. This is nothing as a conjuring trick to a mass of ice plucking out fragments of the floor on which it is meanwhile pressing with a pressure of several hundred tons to the square yard.

This is an *a priori* argument, but it can be supported in another way. If the process of disintegrating its bed and plucking out the fragments is a possible one, there is surely no place anywhere where it ought to be more continuously and effectively done than in Greenland, where the mass of ice is greater than it is anywhere else in the world, and where its motion and therefore its dynamical energy are the greatest also; but, as I have just now said, in Greenland there are no moraines where there are no *nunatakker*. This seems to be conclusive.

Again, it has been observed in many places that the striæ often show cross hatching on the same rock surfaces. I will quote a few examples. J. Geikie speaks of rocks that exhibit a "cross hatching" of striæ and where the striations on two contiguous rocks do not agree in direction, while the till shows an intermingling of stones derived from separate districts (*Great Ice Age*, p. 77). Again, he says, "between Kishorn and Applecross excellent examples of cross hatching are visible, the older set pointing west 30° north, and the latter south 28° west. In the mountain district of South Ayrshire, which was mapped for the Geological Survey by Messrs. Peach and Horne and myself, similar phenomena are conspicuous" (*ibid.*, p. 262). "Examples of such cross hatching of striæ occur, as my friend Mr. Jack has proved, very abundantly in the basin of the Clyde." "Near Grange and at Keith Mr. Horne detected excellent examples of cross hatchings, one set of striæ pointing south of east, and the other north of east" (*ibid.*, pp. 828-829). Again, "at Rudersdorf (Berlin) there are two sets of striæ, one set trending towards south south-east, the other and later series being directed towards the west" (*ibid.*, p. 426). In Finland, Rosberg tells us that "two distinct systems of glacial striæ are apparent . . . the later striæ crossing the earlier ones at various angles. When striæ belonging to both systems appear on one and the same rock surface, the younger are always the fresher of the two, the older ones being worn and abraded. Reference may be made here to the similar occurrence of two sets of striæ in the island of Gothland, the older trending from north-east to south-west, the younger from north to south or a little north of north to east of south. . . . Similar observations have been made on the coast lands of Norrbötten, between Piteä and Luleä, where two systems of striæ are seen, one of which trends from north-west to south-east, and the other from north north-west to south south-east" (*ibid.*, p. 474). "On the exposures of the chalk of Faxö in Seeland three systems of rock striæ have been described by Forschhammer." "The distribution of the erratics of Faxö chalk," says Geikie, "is quite in keeping with the evidence of the striæ. . . . Again, two systems of striæ are met with in Askö and Svansberg (Laaland), one from south-east to north, and a second from east north-east to west

south-west; while at Lellinge the direction is from east north-east to west south-west" (*ibid.*, p. 478, note).

"It is noteworthy," says J. Geikie, "that in North America there are sometimes two or more sets of striæ and groovings. In the district of Lake Superior, for example, there are traces of two distinct glaciations, the main or continental glacier having flowed from north-east to south-west, while the local and latest ice-flow was from north to south. . . . In Western New York there is in addition to the south-west system a subordinate south system (Hall), and on Isle la Motte in Lake Champlain there are eight sets (Adams), although usually not over two or three in Vermont. . . . Near Lake Winnipeg and the Lake of the Woods there appear to be at least two sets of striæ, according to Mr. J. W. Dawson" (*Great Ice Age*, 2nd ed., p. 456, note).

Sir Wm. Dawson writes at some length on this question, giving examples from Canada, and says generally that "it is not infrequent to have two sets of striæ nearly at right angles to each other in the same locality," and adds that, in a fine exposure recently made at the Mile-End Quarries, near Montreal, the polished and grooved surface of the limestone shows four sets of striæ (*The Canadian Ice Age*, pp. 42, 45).

There are striæ on the silurian limestones near Point au Pique which run about north 45° west, but these are crossed by another set having a course south 30° west. At Lake Nipigon, near Lake Superior, Mr. R. Ball found a set of south-west striæ crossed by a south-east set.

R. Ball, speaking of the glacial phenomenon of Canada, says: "When the direction of the striæ in all parts of the country are laid down upon a map, some degree of parallelism is shown within the various groups, yet the general bearings of these are so different that it is difficult to see how they could all have been produced contemporaneously or by a confluent ice-sheet; and yet, excepting far to the north, they all seem to be equally old and to have the same relations to the till. If any great interval of time had elapsed between the production of these various sets of grooves, we should see greater differences among them than we do." Ball compares this with the fact of the distribution of the blocks of white quartzite matrix with red jasper pebbles found at the east end

of Lake Superior, and he remarks on its wide lateral dispersion from a common centre and partly across the direction of the existing striæ (*Bull. Geol. Soc. Amer.*, i., pp. 301-302).

This cross hatching seems to me to be a very awkward fact for the ultra-glacialists. Mr. Geikie tries to explain it in two ways. On page 77 of his work above cited he attributes it in certain places to varying directions in the currents of ice, and in others to two different periods or stages of glaciation. It seems to me very difficult to understand this. I do not myself profess to understand how, with the same contour of country, the same lines of drainage, and the same lines of least resistance, a very viscous substance like ice, which moves by gravity alone, can at one time move in one direction and at another in an entirely different direction in the same district. This appeal seems to me to be to transcendental physics and not to the laws of matter as we know it. Apart from this I cannot see how the striæ can have been made at two different glacial periods, as is alleged.

Unless the whole theory of glacial erosion has been outrageously exaggerated in its effects, it seems to me quite impossible to doubt that the whole effect of the first glaciation, in so far as it was represented by these fine lines and striæ, must have been weathered away and worn off before the second glaciation had completed its work and left its touch upon the rocks in the shape of another set of fine lines and striæ.

To my Philistine judgment the occurrence of these cross hatchings where they are found is evidence that whatever made them must have done it at one time, and that in doing it, it was sufficiently mobile in its touch to enable one set of gravers to work in one direction and another in another, and that no kind of ice under any conditions known to me could have this mobility.

Again, it was the opinion of two very keen observers of so-called glacial phenomena, Mr. Campbell of Islay and the late Sir Wm. Dawson, that in the North-West Highlands and in the islands of Western Scotland on the one hand, and in large parts of Canada on the other, the striations distinctly point to the motive force which made them having come not from the high land moving towards the sea, but that it moved from

the sea inland. I will content myself with quoting at present from the latter. He says: "I have no hesitation in asserting from my own observations as well as from those of others, that for the south-west striation the direction was *from the ocean towards the interior against the slope of the St. Lawrence valley*. . . . This at once disposes of the glacier theory for the prevailing set of striæ; for we cannot suppose a glacier moving from the Atlantic up into the interior" (*Acadian Geology*, p. 69).

Again, there is the argument from the incompetency of ice to act as a vice for the tools necessary to fashion the striæ, even when it is hard blue ice. I do not dispute that a glacier, when it encloses a stone or a layer of sand and presses abnormally against one of its walls, or when it is in a tight place in turning a corner, can and does cause striations for a foot or two or a yard or two in length, but this is not what we have to explain. What we have to explain is, how ice, even the hardest blue ice, which is a yielding substance and which, when it presses on the graving stone, does not act like a rigid body, but allows or rather compels the stone to penetrate into itself and cannot hold it in a firm grasp, can take any particular stone up and carry it along in perfectly straight lines for scores of miles when acting under the differential pressures involved in a journey over hill and dale. Even when toughest and hardest at the very bottom of a glacier, as Spencer and others have shown in Norway, ice acts towards the stones it meets like wax does to the seal which presses into it and allows itself to be moulded upon it. If its hold is so slight as only to allow of a very short and occasional striation being made on a sloping bed in a modern glacier, how can we conceive such a thing possible to the extent of miles of engraved parallel lines in the case of ice supposed to be moving, not with gravity helping it, but over a flat surface, or up hill, against gravity therefore, and doing this entirely by a drag on its nether surface caused by the successive viscous flow of the layers of the ice over each other?

These and similar arguments have been used, and used again without avail, by the present writer. No attempt has been or is made to meet them or answer them, or to give any rational explanation of how the postulated process could be

made to work under any known conditions, I mean conditions known to mundane physics. The monotonous reply is the same that was made to their admiring votaries by the priests of Diana, the great mother goddess of Asia Minor: "Great is the Omnipotent Ice Goddess of the modern Ephesians who carry the banner of geological orthodoxy".

There is no pretence for saying that the striæ in question could only be produced by ice, since every other cause is impotent, and that we are therefore forced by a process of exhaustion to turn to this one in spite of its overwhelming difficulties. I contend that this is quite preposterous. All that we need for making striæ is a set of graving tools and a motive force. The graving tools are available in the stones and sand under any hypothesis, while, in regard to the motive force, water is much more efficient than ice in every way. Ice is, in fact, only very viscous water, and the very difficulties which attach to such a viscous substance as ice vanish when we get a substance so mobile as water—granting always, of course, that it is water in large masses and in rapid motion. Under these conditions no motive force is more potent. No vice is firmer in its grasp than a torrent of rapid water driving a mass of gravel and pebbles and sand, and no dynamical engine is more powerful than a torrent. There is no excuse whatever, therefore, for limiting the appeal to the ice monster as the only possible explanation of the difficulty, but the reverse. It is because the ice monster is quite incompetent to do the work that an appeal must be made elsewhere, and why I so persistently have appealed to water as against ice as the real fashioner of the drift phenomena, and notably of the striæ so persistently referred to as glacial.

Let us now move on and say a few words about the manufacture and cause of the angular erratics and the angular drift.

The explanation of the origin and manufacture of these stones seems to me a great difficulty for the glacial champions. In some respects, and in some only, they have a parallel history to the rounded boulders. They occur of all sizes, from enormous masses as big as a house down to the pebbles in the angular drift, and they are scattered through and upon the drift beds in very much the same way, and whatever

their origin they seem unquestionably to have been deposited by the same force and the same impulse as the rounded and rolled stones.

As we have seen they differ from the rounded boulders and pebbles in having their angles more or less sharp and unrubbed, and they cannot therefore have passed through the same mill by which the rounded stones were shaped and formed. If they have travelled far it must have been rapidly, or under conditions when continuous rolling for long periods was not possible. The mode in which they were carried I will try and discuss in another chapter. At present I am only exercised to discuss how they were made. To the ultra-glacialist, who never seems to me to verify his premises, it is the simplest of questions. He merely says these stones were split and splintered by the frost and cold of the ice age, and this very elementary statement has been repeated galore. To me, and perhaps to others, no explanation could be more ridiculous and farther from the facts of observation.

Let us try and realise how frost works to split stones. The only way in which it works, so far as I know, is by alternately freezing and thawing them *in cases where crevices or fissures or cracks are available* for the moisture to get access to the interior of the stones. I lived many years in a part of Lancashire where the process has been utilised for generations by the quarrymen who get the flags used in our causeways. The stone is there quarried in large blocks in the summer and spring, and the moisture finds its way (or has done so long before when the stones were *in situ*) between certain laminæ generally marking old surfaces. The great blocks are then exposed to the winter's frost, which freezes the films that have got between the laminæ, expands them and thus splits the blocks into flagstones. The process is simple and obvious. The frozen water is simply a form of wedge. In many cases the blocks won't split at all, however, because there are no natural faces to the laminæ, and these masses of dead rock lie about the quarries as examples and warnings. While, if we attempt the process with rocks of other kinds whose masses are crystalline or homogeneous in structure and not laminar, rocks like granite, gneiss, limestone or coarse sandstone, we may expose them as long as we please to frost but there will be

no splitting. There will, in the course of time, be a certain surface decay and disintegration in some of them, due to the alternate expansion and contraction caused by the alternating temperatures of our summers and winters, but no splitting into angular blocks or fragments.

What we can thus see at work in our Lancashire and Yorkshire flag quarries is what takes place in nature on a large scale when masses of rock are permeated by cracks and fissures and joints if they are accessible to moisture. These rocks when exposed to the elements are naturally worked upon by the winter frost, and the already separated blocks or those separable in this way are easily detached. In this way, no doubt, we can account for many of the angular blocks in the Alps, in the Dovrefelds and other high mountains. They have come from the exposed peaks which have been disintegrated in this fashion, but this explanation can only apply to a very small section of the angular blocks, and to hardly any of the angular pebbles in the gravels. These have not been detached along lines of jointure, but have been split and broken off the bed rock and still show the crystalline and irregular structure of the broken faces. No frost, however keen and prolonged, could effect this kind of breakage in gneiss and porphyry and greenstone and other hard rocks like these; much less could it fracture flints into myriads of angular bits as we find them in the angular gravels. Frost seems to me quite an inefficient weapon for such a task, even in case of alternations of temperature such as might be postulated where winters were very cold and summers very hot, but the very essence of the glacial theory excludes such alternations. The ground in the so-called glacial age, by the hypothesis itself, was wrapped and mantled in ice and snow in summer as well as winter. That age was avowedly a long winter of the most savage kind, and this being so, this kind of alternation of temperature, even if it were an efficient cause, is not available, and becomes a quite paradoxical hypothesis.

We may subject flints and quartz and quartzite pebbles or blocks to the efforts of frost for generations without cracking or splitting them, nor do I in fact know how it could split them under any conditions so long as they remained without internal fissures into which it could drive its wedges,

and the same is the case with the great masses of porphyry, porphyrite, greenstone and gneiss which have been smashed right across their crystalline structure and along no joints or faces.

There is only one way, it seems to me, in which these splintered and angular stones could have acquired their present shape, namely, by violent impact or force; and I know of no force available except the throes and strains of the earth when remodelling its crust, which have occurred so frequently in its history when mountains and valleys were being made, and pinnacles and pics were being shot into mid-air: the same kind of forces which tossed the Alpine tertiaries on end, and rolled the chalk of Northern Europe into its successive Downs. It was when the rocks *in situ* were being bent and twisted and riven and shattered and torn by sudden or rapid and perhaps explosive forces that the vast breakages took place of which we see the numerous débris in the angular masses of riven rock we find in so many places.

When such forces acted upon a matrix of a softer and more yielding material, and enclosing hard and brittle nuclei or other masses, when they in fact acted upon a substance like chalk, containing irregular or tabular masses of hard and brittle flint in its tight grasp, they would, no doubt, shiver and break the contained flints into angular fragments, leaving the more yielding chalky matrix largely intact.

As a matter of fact we know that wherever the chalk-with-flints has been largely disturbed *in situ*, there the flints it contains are generally shattered and splintered. Very fine examples are to be seen in the Isle of Wight, and especially at Freshwater Bay.

The view is not new; thus we read: "In certain places the flints are all fractured although retaining their original form and situation. Sir Henry Englefield was the first to recognise this at Carisbrook in the Isle of Wight, and suggested that it was due to some sudden shock or convulsion which in an instant shivered the flints, though their resistance stopped the incipient motion, for the flints though crushed are not displaced" (Mantell, *G. S. England*, pp. 82, 83).

Again we read: "Another form of angular gravel is formed of shivered chert from the greensand. The chert seems to

have split *in situ*. A remarkable example may be seen in the cliffs overhanging the new road between Lyme and Charmouth, and also on Abbotsbury Castle. In the oolite also, two miles west of Bridport, a similar dislocation and splitting of the stones has converted to loose breccia the upper beds of inferior oolite on part of Chideock Hill. . . . It is difficult to distinguish the deposits of angular breccia, both of chert and on the chalk formation, from deposits of diluvial gravel which have been moved only a short distance. The cliffs at Axmouth and near Sidmouth exhibit deposits of such miscellaneous diluvial gravel resting on red marl and adjacent to beds of uniform and angular chalk flint breccia that rest on the chalk" (*Geol. Trans.*, 2nd series, iv., pp. 7, 8).

If, on the other hand, we follow the angular boulders of crystalline rock, which occur in the German drifts and in Southern Scandinavia, to their parent beds, which we can easily do, as I have done, since the migrant lines of such angular blocks can be traced right up to Dalecarlia, and we shall come, as I shall describe presently, upon such scenes of ruin and violent breakage as are consistent only with the earth movements and strains I have mentioned: a conclusion which my master, Murchison, with his eagle eye was constrained to accept and urge when he visited and described this district.

The same argument applies to the heaps of angular blocks piled up in many places elsewhere, and notably on the top of Scaw Fell.

I shall show in a later chapter that the distribution of the drift was coincident with vast dislocations of the chalk and other strata over wide areas, thus furnishing the only efficient cause, and no doubt, the true cause, of the greater part of the angular drift, great and small; and it seems clear that just as in the case of the rounded and rolled drift the force which distributed the stones found them ready-made and did not make them, so also in the case of the angular drift the materials, though not the result of long and secular changes, were made and ready to its hand when the force that scattered the drift was at work. The only difference was, that in the one case the rolled stones were of tertiary or earlier date and had been manufactured long before, while the angular drift had only

been recently or concurrently made. The two sets of stones, as we know, were mixed and mingled and borne along together, but their mode of manufacture was different, and this and this only is the explanation of the fact of their occurring together as they do. Thus, when we are asked to explain the drift, it is clear that the problem we have to solve is a much simpler one than has been supposed. It does not involve an explanation of the making of the rolled stones and of the angular drift of the gravels and their contained pebbles, nor of the sands and the clays. These had been manufactured by other processes, and many of them long before. What we have to explain is a much simpler matter, it is merely the mode of distribution of these materials as we now find them distributed, and the question we have to face is: Did ice in any form or shape, either as ice-sheets or ice-glaciers or ice-bergs, distribute them (could it in fact distribute them as we find them), or was this done by some other agency? To this, the crux of the inquiry which has occupied us in previous volumes, I will again turn in the next chapter.

CHAPTER XV.

THE INTERNAL STRUCTURE OF THE DRIFT BEDS AS EVIDENCE OF THEIR ORIGIN AND DISTRIBUTION.

"When it becomes necessary to invent imaginary conditions to do imaginary work instead of rigorously reasoning out the probabilities of geological facts—all too few in many cases—I shall leave the seemingly congenial occupation to the poets and romancers of science, and confess myself entirely unfitted for the prosecution of scientific investigation" (T. M. Reade, *Geological Magazine*, 1893, p. 37).

IN the two previous chapters I have tried to reduce the problem of explaining the drift to much smaller proportions than is usually done, and to argue that a large class of phenomena, which have been often confused with that problem, have nothing to do with it, but must be explained by a reference to an earlier geological period, or to other and separate causes.

With the exception of certain striæ, a limited number among the great mass of striæ generally classed together, and which seem directly connected with the movement of the drift when it was last distributed, the phenomena discussed in the previous two chapters seem to have no direct connection with the problem supposed to be involved in appeals to an Ice Age, or to other explanations of the drift, I mean the grinding down of rocks into boulders and gravel and sand and clay, and the rounding of rock surfaces so often overlaid by drift. These are all most important geological riddles, but they are important to the student of Tertiary Geology and not of the so-called drift period. The work they testify to was done in tertiary times or earlier.

Let us turn, therefore, to what is more germane to our question, namely, not to the manufacture of the materials of the drift but to their distribution, a question which has occupied us largely in former works. This is the real drift

problem, and it presents itself in two ways; that is to say, we have to inquire into two features or factors of the distribution. First, the internal structure of the drift beds; and secondly, the external contours and features of the same beds.

Lyell, who first classified these beds, divided the drifts of Norfolk into two sections, the stratified and the unstratified drift. To the latter he gave the name of Till, a name by which the country people in Scotland call a similar deposit. It would have been well if the term till had been strictly limited to this, its original meaning, as it generally is by American geologists. Its essential feature is that it is unassorted and void of structure.

Let me give a description of it from one who knows it well, namely, Prof. James Geikie. He describes it as "usually a firm, tough, tenacious, stony clay, which gives every evidence of having been subjected to great pressure. So tough, indeed, does it often become that engineers would much rather excavate the most obdurate rocks. . . . Often, however, the clay becomes coarser and sandier, and when this is the case water soaks through it. It then loses consistency, and is sometimes ready to 'run' or collapse as soon as an excavation is made. Again, in certain districts it might be described as a coarse agglomerate of sub-angular and angular stones set in a scanty matrix of coarse, earthy grit and sand, as in the lower tracts of many of the islands of the Hebrides. A still more abnormal kind of till is found occasionally in sandstone districts. It consists of an exceedingly rude débris of the underlying sandstone, intermingled with which occur a few stones derived from a distance. The sandstone blocks are of all sizes and shapes, from two or three inches up to fragments five feet and more in diameter. In some places they are heaped confusedly together with little or no matrix in the interstices between the blocks. In other places, however, we find the stones set in a more or less meagre matrix of clay and sand. This rude kind of till sometimes attains a thickness of more than twenty feet, as in the moors of Kirk of Shotts, Lancashire, etc. It passes gradually into till of the normal type.

"Sometimes the stones in the till are so numerous that hardly any matrix of clay is visible. This, however, does not often happen. On the other hand, they occasionally appear

more sparsely scattered through the clay, but this occurs still less frequently. As a rule, it is hard to say whether the stones or the clay form the larger percentage of the deposit in a mass of typical till, its stony character being generally more pronounced in hilly districts, but to this there are many exceptions. . . . Stones and boulders alike are scattered higgledy-piggledy, pell-mell through the clay, so as to give to the whole deposit a highly confused and tumultuous appearance" (*Great Ice Age*, third edition, pp. 7-10).

This being without question the most typical of so-called glacial deposit, it is a remarkable fact that no such deposit is now being made, so far as we know, by land-ice anywhere. In my *Glacial Nightmare* (pp. 685-693 and 777-778) I have quoted the observations of several observers on the subject, and will now quote one or two more.

Prof. Bonney surely pronounces the conclusion of all of us who have visited Alpine countries containing glaciers when he writes as follows: "The glaciers of the Swiss and Savoy Alps have been retreating for several years; if anything like ground moraine existed this would be a very favourable time for observing it. *In no case have I been able to find signs of any deposit resembling till or boulder clay.* . . . Nothing has been seen but bare rock, with now and then a film of mud or a passing stone. In short, the result of an experience of some years has convinced me, that if anything like the till or ground moraine of recent glacialists exists in the Alps, it is a very local and exceptional phenomenon."

In regard to Greenland, Nordenskiold describes an area lately left by the ice as containing no moraines, and in his description makes no mention of boulder clay. "We passed, in fact, over ground that had but lately been abandoned by the inland ice. . . . Everywhere occur rounded but seldom scratched hills of gneiss with erratic blocks in the most unstable positions of equilibrium, separated by valleys with small mountain lakes and scratched rock surfaces. On the other hand, no real moraines were discoverable. These seemed to be commonly absent in Scandinavia, and are, generally speaking, more characteristic of small glaciers than of real inland ice" (*Arctic Voyages*, pp. 169-170). And in describing the clay beds of Greenland he says: "The material of the

clay beds has evidently been deposited by the glacier rivers, but in general the deposits are sea formations, *i.e.*, they have been deposited under the level of the sea" (*Geol. Mag.*, ix., p. 410).

Similar remarks may be made with regard to those areas in America where a retreat of the ice permits an examination of the ground recently occupied by it. In the *Amer. Journ. of Science* for March, 1902, for example, is an interesting description of Mount St. Elias and its glaciers. Where the ice has retreated glaciated surfaces are seen, but no boulder clay. Spread out, however, over the whole area between the ice and the sea is a mass of stony morainic matter. Streams from the glaciers carry the finely ground rock matter into the sea (Bulman, *Geol. Mag.*, 1892, pp. 306-307).

The position here maintained is quite admitted by Prof. Geikie. Having dogmatically affirmed that the scratched stones in the till undoubtedly owe their shaping and scratching to the action of ice, he says: "The frontal moraines of glaciers do not at all resemble till or boulder clay. The moraine consists for the most part of a confused heap of angular stones and blocks and loose sand and débris; scratched stones are decidedly in a minority, and, indeed, a close search will often fail to show them. Clearly, then, till is not of the nature of a terminal moraine. . . . We look in vain among the glaciers of the Alps for such a deposit. . . . It is clear that the conditions for the gathering of a stony clay like till do not obtain (as far as we know) among the Alpine glaciers" (*Great Ice Age*, pp. 63-65). This is quite frank and honest. It exactly concurs with everything I have myself seen both under and at the foot of Alpine glaciers, but it does not raise any doubts in Mr. Geikie's mind that the till was nevertheless the product of land-ice. Let us follow his argument.

Comparing an ordinary Alpine moraine with a deposit of till, he says of the former, "The scratched stones we may occasionally find, but where is the clay?" He then goes on to describe the milky-white or yellow-brown stream which flows from every glacier, and says: "If we lift some of the water in a glass and examine it, we shall find that its colour is due to the presence of a very fine impalpable mud. In the more sheltered reaches of a glacial river this mud will occasionally

accumulate to some depth. It is an unctuous, sticky deposit, and only requires pressure to knead it into a tenacious clay. There can be no doubt whatever that it owes its origin to the grinding power of the glacier. The stones and mud which the ice forces along are crushed and pulverised against each other and upon the rock below, and the finer material resulting from the action is what renders the glacial rivers turbid and milky. *If there were no water to wash out the mud formed in this way below the glacier, it is evident that not only* scratched stones but clay also would gather underneath the ice and be pushed out at its termination; and this clay, owing to excessive pressure and to the finely divided nature of its ingredients, would be hard and tough. The till of the Scottish Lowlands, when it has been exposed to the influence of the weather, sooner or later crumbles down, and, when water rushes over it, then that which was once a hard tough clay becomes a soft, sticky, unctuous mud that clings persistently to everything it touches. No one who compares this mud with that derived from the glacial waters of the Alps will fail to notice their similarity." Mr. James Geikie then goes on to argue that "the upper reaches of the Swiss valleys are, as a rule, too steep, and there is too much water circulating below the ice to allow any considerable thickness of such a deposit as till to accumulate," and after urging objections to an appeal to icebergs as a cause of the till, he appeals to vast ice-sheets not moving down hollowed valleys but marching over level country or over a rolling country, irrespective of the slope and gradients of the ground. These ice-sheets would, it is argued, produce quite a different product in the shape of a moraine to those produced by glaciers. Mr. Geikie thus describes what he imagines was the case: "As the crushing and grinding continued, few stones would escape being smoothed and striated, while the fine mud resulting from all this work would get mixed up with the stones and form a stony clay. It is true that water would circulate below the ice to some extent, as we know it does underneath the glaciers of Greenland, and no doubt much glacial mud would be carried away by this means; nevertheless all that could possibly escape would bear but a very small proportion to what remained behind. Thus both mud and stones would

tend to collect under the ice, and as that great mass moved onwards, pressing with prodigious weight, the mud and stones would be squeezed and dragged forward so as to become a confused and pell-mell mixture of clay and stones with here and there traces of water-action in the form of irregular patches and interrupted bands of stones, gravel, earthy sand and clay—in short, till or boulder clay. Such, then, would appear to be the origin of that remarkable deposit; it is the ground moraine, *or moraine profonde* of the old ice-sheet" (*ibid.*, p. 65, etc.).

The view here urged is that held by the orthodox school of glacial geologists. I will only add to it a corresponding paragraph from the late Prof. Green's text-book. The italics have been added to point the moral more plainly. "We have already seen," he says, "that under such a sheet (continental ice-sheet) there is *probably* found an accumulation of clay and stones known as *moraine profonde* or *grund moraine*, and till resembles exactly what we picture to ourselves that this deposit *must* be like. . . . Though the existence of *the moraine profonde* is to *a certain extent hypothetical*, the *probability* that such an accumulation is found beneath great ice-sheets is so great, and its character, *if it exists, must be so exactly* that of till, that nearly all geologists are now agreed to look upon the latter as having been formed by the grinding and wearing away by an ice-sheet of the ground on which it rested" (*Manual of Geology*, p. 264).

It will be seen by any one accustomed to inductive methods in science that in all this we have abandoned real induction almost entirely, and have fallen back upon those methods of deductive logic which still prevail so much in Germany and which consist in piling up one hypothesis upon another, all derived from the land of clouds in order to sustain some *a priori* postulate.

Let us test the position at some other points. In the first place in regard to Alpine glaciers, Prof. Geikie seems to think that water is continually circulating underneath the whole of a glacier. The fact is, a glacier stream or river is a mere drain carrying off the melted surface water that reaches it through crevasses, and the greater part of a glacier is as tightly pressed against its bed as a mass of such

a weight which can accommodate itself to its bed must necessarily be. In a great number of Alpine glaciers the sub-glacial stream occupies only a very small part of the glacier bed. The rest is precisely in the same condition that that of the supposed ice-sheet was in, with this exception, that since a glacier's motion is faster its erosive tendency must be greater than that of the postulated ice-sheet. This being so, how is it that even in the widest Alpine valleys, take that of the Rhône glacier for instance, we have nothing like till, but a part of the bottom of the valley below the glacier is strewn with clean-washed rounded stones and the rest is mere bare rock, while when we creep underneath the ice we merely have a smoothed bed and no mantle of soft deposit over it? The argument of Prof. Geikie in this behalf is surely altogether irrelevant.

If we leave Switzerland and examine areas where the masses of ice are very much larger, and are, in fact, the only masses that can be compared in size and bulk with the supposed ice-sheets, namely, Greenland and Alaska, the fact is even more patent.

In regard to Greenland, the most like an ice-sheet of any great mass of existing ice known to me, I can find no evidence whatever of any such ground moraine or sub-glacial till as Prof. Geikie speaks of. Nordenskiold and all other geologists who have explored the country have, so far as I know, utterly denied the existence of anything like it.

Chamberlin, whose prejudices are all the other way, is constrained to say: "The amount of drift on the territory once occupied but now free from ice in Greenland is notable rather for its scantiness than its abundance. On Disco Island it was found to be very limited, except along the immediate fronts of the present glaciers. In the Inglefield Gulf region there are at some points very considerable accumulations of drift within a mile or two of the present ice-front, but at the same time much of the territory between the ice-front and the sea bears a very scanty covering of drift. No great moraines were seen, nor any thick mantles of drift. The valleys in front of the glaciers are well glossed with glacial work, but even here the rock occasionally appears" (*Recent Glacial Studies in Greenland*, p. 217). Nordenskiold

denies the existence of any true moraines in Greenland. They are also commonly absent from Scandinavia, and seem, as he says, more characteristic of small glaciers than of inland ice (*Arctic Voyages*, pp. 169-170). No glacial clays occur on the strip of Greenland coast lying between the inland glaciers and the sea. At Mount St. Elias, in North-Western America, no boulder clay occurs (see *Amer. Journ. of Science*, March, 1892).

Let us now look at the case from some other aspects. A considerable part of the stones contained in till seem to me to bear unmistakable traces of having been rolled in water. Only a portion of them have their angles sharp; on the contrary, they have their edges blunted and rounded. How could this happen if the till in which they are contained were the product of a frozen mass of ice grinding down, by means of stones embedded in its substance, the underlying rocks? We can understand how this could happen in a glacier where some of the stones are carried intact on its back and some are rubbed against its floor. But we have not to do here with glaciers. It must be remembered that if we substitute ice-sheets for glaciers on the scale required, we must bury far down in the ice all the rocky surfaces from which the stones in the till could be derived. This is quite admitted by Prof. Geikie. Thus he says: "In the glaciers of the Alps we have every reason to believe that a considerable proportion of stones used as chisels and stylets by the ice are introduced from above; they tumble from the crags upon the surface of the ice, and drop into those deep crevasses which must sometimes cut a glacier to its bottom. But when ice buried Scotland to a depth of several thousand feet, only a few hill tops would rise above the general level of the *mer de glace*. Consequently, little débris would be showered upon the ice; and even supposing considerable heaps of blocks and rocky rubbish did accumulate here and there at the base of some isolated hill, it is nevertheless very unlikely that any portion would ever work its way to the bottom of the thick ice-sheet. The gravers employed by the ice in dressing the Scottish hills and valleys could not have been derived from above; they must have been obtained from below" (*Great Ice Age*).

This being Geikie's view of the origin of the great mass of the drift, I cannot see how he can explain the difficulty with which I started the last paragraph, namely, the mixture of rolled stones and angular ones (both of them far travelled erratics) in the drift. Apart, however, from the difficulty of explaining a mixture of angular and rounded boulders by means of an ice-sheet, that hypothesis (first started, I believe, by E. Collomb and warmly supported by Penck and others) at once brings us face to face with the paradox I have already analysed and I think shown the complete absurdity of, and which involves a capacity in ice for digging up or breaking up its own floor. As it seems to me, Greenland is a perfectly unanswerable test of such notions. There we have virtually no moraines at all where the biggest glaciers flow out, unless there are exposed crags (*nunatakker*), and in Greenland, as I have previously said, the effective denuding force must be much greater than in any possible ice-sheet because the gradient of the glaciers is so steep and their ascertained speed is so great. There, if anywhere, the tooth-drawing process of glacial stone-gathering ought to flourish. How such a process which does not exist under the favourable dynamical conditions of Greenland should be possible when ice is moving over level plains by means merely of its viscous drag, and therefore moving very slowly and ineffectively, I know not.

In regard to the more general question as to the possibility of so-called ground moraines, and the tremendous and, in fact, insuperable difficulties they present, I can only refer to my former analysis and the opinion of many competent observers of the question, which have in no point been met or answered by the ice men (*Glacial Nightmare*, pp. 687-693; *ante*, vol. i., p. 409). So strong and overwhelming is the case against ground moraines, however, that in America the champions of the ice age are giving it up altogether, and falling back on their notions about *englacial drift*, etc., which I have already criticised. It is plain, therefore, that if we are to follow inductive methods, and to explain the till and its origin by any process of ice-work of which we have at present cognisance, we must discard ice, in the form, at all events, of land-ice, as a quite incompetent instrument.

On the other hand, it seems to present every feature which

we should expect to find in a subaqueous deposit, and the only deposits at all like it now being made are subaqueous. On this part of the issue again I have written at some length in my *Glacial Nightmare* (pp. 778-779). I will add one or two more testimonies to those I have there quoted.

Prof. Dawkins says: "The hypothesis that the boulder clays have been formed on land is open to the objection that no similar clays have been proved to be so formed, either in the Arctic regions where the ice-sheet has retreated, or in the districts forsaken by the glaciers on the Alps or Pyrenees, or in any other mountain chain. Similar deposits, however, have been met with in Davis Strait and in the North Atlantic, which have been formed by melting icebergs; and we may therefore conclude that the boulder clays have had a like origin" (*Early Man in Britain*, pp. 116-117).

Again, Sir Wm. Dawson says: "The results of the investigations of the *Challenger* in the Antarctic Ocean are of great importance with reference to the formation of marine till and stony clays. The dredge may now be said to have settled the question by ascertaining the deposition of marine boulder clay, or at least of a deposit of sand and clay, with fragments of various rocks over areas perhaps as great as those now covered with similar deposits in the northern hemisphere. It is most instructive to find that a bed of this stony mud is in process of deposition from floating ice in the southern ocean, and this with such rapidity that the *foraminifera* and other organisms elsewhere forming the deep sea ooze are quite masked by it" (*Ice Age in Canada*, pp. 100-101).

A more powerful advocate of the same view is my friend Colonel Fielden, whose well-known paper on the deposits of the Island of Kolguef seems almost conclusive on the subject.

In this very interesting paper, describing the stones exposed by the streams in the beds, he says: "An examination of these stones, which are of every shape and form, from angular fragments to rounded and polished blocks, shows that a large proportion are ice-scratched" (*say scratched, H.H.H.*). "The medley of rocks represented is remarkable, granites and gneisses, limestones, silurian and carboniferous grits, quartzites, porphyries, a variety indeed so great that it would take a trained petrologist to enumerate them. An immense land

surface has been put under contribution to supply such a diversity of rocks. The boulders are of all sizes, from a walnut to large dimensions; one of hard yellow sandstone was polished, scored and striated. Along its major axis it had deep flutings cut into it, and in addition it was transversely scratched. It measured fifteen feet in length, nine feet in breadth and six feet in height. In the mud and clay cliffs are here and there stones and boulders of the same character sticking out of their banks or resting on the talus ready to slide down. These erratics have no tendency to form lines of horizontal deposit in the beds. My opinion," he says, "is that all have been dropped from floating ice intermittently and tranquilly. The matrix of clay around them shows no signs of disturbance; on falling from the ice-rafts they have sunk gently into the yielding mass, and now they lie throughout the beds looking like currants in a cut loaf." He adds that he has no altogether satisfactory explanation of the presence of such immense numbers of ice-scratched stones as occur at Kolguef. "*The action of an ice-sheet cannot be invoked at Kolguef.*" He further asks where the millions of scratched stones found there can have been manufactured, and speaks of the difficulty of conceiving a train of circumstances admitting of erratics being transported in floe-ice to some rocky coast, there to be scratched and carried out to sea and deposited on the floor of the ocean, and if they came from glaciers, how did ice-scratched stones get from the glacier to the ice-raft? Where, however, could such glaciers have existed at the time when these beds were being laid down?

"The Kolguef beds, like the so-called glacial beds in the East Anglia cliffs, may be divided into clays and sands, but their differences of composition merge into one another. Not unfrequently the clays pass into horizons of a more sandy composition, although so insensibly that it is difficult to determine exactly where the change takes place. The cliffs are so homogeneous in character, and the passage of the clay stratum into sand is so gradual, that the alternation is evidenced more by the change of colour in the sections than by any definite lines of demarcation. It is evident that no break has occurred in the continuous deposition of these

sedimentary beds. . . . The Kolguef beds were evidently deposited in the sea. . . . Various fragments of *Saxicava arctica, Mya*, etc., appeared to be dispersed from the bottom to the top of the beds; they were rare, and I obtained but few entire examples. . . . These molluscan remains, found in various localities and through the whole exposed thickness of some of the beds, all of which, are in my opinion, sedimentary, afford proof of their marine origin."

Colonel Fielden, in comparing the boulder clays of England with those of Kolguef, says: "To a certain extent all those that we have at home are fragmentary when compared with the boulder-bearing beds of Kolguef, which we may assume are fifty miles in length by forty in width, with a thickness of not less than 250 feet, probably far more, all lying in one undisturbed mass without the slightest sign of a basement or interrupting rock," while he tells us that he met at Kolguef with no deposit precisely like that which is called till in Scotland, *i.e.*, a tough, firm clay with neither crack nor joint, which will not blast, and is difficult to pick to pieces. He goes on to say that there are many deposits in Britain called "boulder clays," which are in no degree superior in toughness to those of Kolguef, for instance those of the Yorkshire coast and the chalky boulder clays of Norfolk. Colonel Fielden suggests that the tenacity of the clays depends very largely upon the nature of the rocks from which they have been derived, and he suggests that many boulder clays in this country, and in other parts of the world, may have been deposited under water. "It is suggestive," he continues, "that all the glacial deposits which I have met with in Arctic and Polar lands, with the exception of terminal moraines, now forming above sea-level, in areas so widely separated as Smith's Sound, Grinnell Land, Northern Greenland, Spitzbergen, Novaya Zemblaya and Arctic Norway, should be glacio-marine beds. Throughout this broad expanse of the Arctic regions I have come across no beds that could be satisfactorily assigned to the direct action of land-ice; that is to say, beds formed *in situ* by the grinding force and pressure of an ice-sheet. On the contrary, so far as I can judge, the glacial beds which I have traced over the extensive area mentioned above have all been deposited subaqueously and re-elevated.

"Nowhere have these facts been more strikingly confirmed than by the investigation of the geological structure of Kolguef. There we find a large island emerging from Barentz Sea, showing, to my mind, evidence by its sedimentary glacio-marine beds of the absence of an ice-sheet from the area when these beds were deposited, and its formation under conditions similar to those which at present exist in Barentz Sea."

In the subsequent discussion Mr. Trevor Battye, who knows the place well, urged two facts. The first one related to the recent elevation of the island shown by the absence from it of the Arctic hare and the lemming, and secondly by the absence of *Saxifraga oppositifolia, Mertensia maritima*, animals and plants of wide Arctic distribution, and further by the absence of *Ledum palustre*, a striking feature of the opposite mainland. He, on the other hand, remarked "that the absence of any continuity of rock *in situ*, the astonishing variety of ice-scratched erratics, the composition and relations of sand and clay, the indefinite line of strata between them, the presence of immense isolated spherical or striated boulders, and the presence of mollusca, showed that all these had been dropped from floating ice, and that the symmetrical conical shape of the isolated hills in the island were due to later erosion by the sea."

Dr. C. H. Hinde considered that the distinctly striated boulders brought by the author indicated that the clays in which they were embedded were of the nature of a genuine boulder clay formed beneath a glacier. The Rev. E. Hill said that the East Anglian clays in area and thickness are comparable to those of Kolguef, their stones are scratched just as those shown.

Colonel Fielden in reply said he could assure the Fellows that the Kolguef beds are as certainly sedimentary beds as the Thanet sands or the thalassic ooze of Barbadoes.

Lastly Prof. Bonney disputed the statement that the ice-scratched stones from Kolguef resembled those of moraines, and said "it was an assumption that the glacial beds of East Anglia were the product of land-ice. It was not yet proved that shell-bearing beds were, or could be, produced by land-ice, and we ought to explain British deposits by those of

Arctic regions, rather than to follow the reverse process," (*Quart. Journ. Geol. Soc.*, lii., pp. 52-65), with all which I cordially agree.

In another paper on the glacial geology of Arctic Europe and its islands (part ii.) Colonel Fielden, speaking of a section of the boulder clay in Tromsödal opposite Tromsö, twenty feet thick, says: "From base to summit it is a homogeneous mass of blue clay, with boulders and stones interspersed throughout. There is not a trace of bedding throughout the mass. I considered this deposit to be a typical example of boulder clay formed under water; it contained ice-scratched stones, and examples of *Cyprina islandica* and *Pecten islandicus*, partially retaining their colour, are common, likewise stones to which the bases of Balanus are attached." Speaking of the islands of Kostin Schar, off the coast of Novaya Zemblaya, Fielden says: "All of these islands which I was enabled to visit have deposits of boulder clay lying in their undulations and hollows. I met with sections showing a depth of twenty feet; the clay is of the same colour as the rocks upon which it rests, and the included stones are angular fragments of the same rock. I did not detect an erratic, or a rounded stone, or an ice-scratched stone in any of this boulder clay. In many places it is full of shells of marine mollusca, *Saxicava arctica* predominating, though I found other species common enough. In some localities one might gather these shells by the bushel, few of them broken, never triturated, and in some cases the two valves are in contact.

"This description holds good also of the part of Gooseland that I visited, the abraded ridges, the deposits of boulder clay in the troughs, and the presence of shells of mollusca, all being characteristic features. As I have expressed a very decided opinion that no ice-sheet has ever extended over this part of Novaya Zemblaya, I may be asked to account for the presence of these widespread deposits of boulder clay with the assemblage of the remains of marine mollusca in them. I venture to urge the view that the wearing down of Gooseland and the islands in Kostin Schar, and the deposition of the boulder clay, are entirely due to the action of floating ice." Speaking again of one of the water-courses in Spitzbergen, the same writer says: "We find sections of mud

and clay rising like walls on either side to a height of from fifty to sixty feet. These beds contain numerous stones, but neither they nor the stones themselves show any signs of stratification; in them I found shells of *Mya truncata*, but in no great quantity. That these beds are of sub-marine formation is confirmed by the existence of raised beaches in the neighbouring fiords and along the adjacent line of coast at a higher elevation than the beds which I am describing. . . . When lifting the ship's anchor from the front of some of the glaciers in North-west Greenland, I have seen it come up with many pounds weight of unctuous mud intermixed with sea shells adhering to the flukes. It is therefore quite evident that water issuing from under a glacier in the Polar regions, and discharging from the ice into the sea, can lay down glacio-marine beds in the ocean, and that the occurrence of ice-scratched stones throughout these beds can be accounted for" (*Quart. Journ. Geol. Soc.*, lii., p. 722, etc.).

This evidence will suffice to show that while nowhere, so far as we know, is anything like boulder clay being directly formed underneath living glaciers, nor even under such glorified glaciers as we find in Greenland, a subaqueous deposit resembling boulder clay in every respect is being formed in high latitudes in many districts, thus pointing the moral we are preaching very directly.

Let us proceed somewhat farther, however.

Mr. James Geikie generally uses the terms till and boulder clay as synonyms, and appeals to both as forms of a ground moraine. Mr. Carvell Lewis, who has written at great length on the same side, has made a clear and definite distinction between what he calls till, *i.e.*, dirty clays or clays mixed with sand, etc., and clean clays, those which have been sorted from the sand or, at all events, exist now separately. He allows to the former alone the character of a true till or land-ice deposit, while he unhesitatingly assigns the latter deposit to water.

Thus, speaking of the extremely tough bluish-grey clay at Ballygory (Rosslare Harbour), he says: "Like other 'tough clay' in Middle England it is very different from the stony, sandy, dirty, heterogeneous dumpheads of drift formed by a land glacier, and to which alone I am accustomed to give the

name of till. Most boulder clays, properly so called, are of aqueous origin formed in water. . . . The laminated boulder clays of Scotland must be of similar origin, also the clays south of Lake Erie and the lower boulder clay of Lancashire" (*Glacial Geol. of Great Britain*, p. 158, note). "The upper and lower boulder clay of Lancashire in the lowlands is also a marine deposit" (*ibid.*, p. 159).

Another conclusion may be gathered from the sand grains contained in the till.

"Most of the glacial beds that I have seen," says Mellard Reade, "bear the marks of disturbance by the tides, in the form of current bedding, especially the sands. The sands are intercalated in the clays, and it is difficult to separate the one from the other. The boulder clays, according to my interpretation, bear the marks of current action also in the smallness and rounded nature of the shell fragments, the roundness of the grains of sand and their high polish, and the manner in which sand beds occur therein. The proportion of sand to clay is generally very considerable, the small gravel is often also highly polished and rounded. Numerous examinations of low level boulder clays by washing, sifting and separation of the grains, impress me very strongly with the enormous wear which every constituent particle of the boulder clay has undergone, something totally different to what is seen in sand washed out of a living glacier, which is uniformly angular."

In all this most students of physics must surely agree. If we are to correlate till in any way with the only products of land-ice which we can examine, namely, moraines, we can only apply the term in the first place to deposits which are unstratified, and, secondly, to those which are *quite heterogeneous and unsorted*. Stratification and sorting can only be the result of the action of water.

While it is quite possible to explain the existence of great masses of unstratified boulder clay as the result of very rapid deposition by water, as we shall show later on, there is no way in which we can understand a stratified deposit being laid down by a hard substance like ice, which could not arrange its subjacent beds in laminæ but only as a crushed and heterogeneous mass. Prof. Geikie seems to have changed his view in regard to the existence of stratified till in the

very arcana of glaciation in Scotland. In the second edition of his *Great Ice Age* he says of the Scotch till: "The clay does not arrange itself in layers or beds, but is distinctly unstratified. We cannot dig it with greater ease in any one direction. It shows no lines of division, but is a homogeneous mass from top to bottom. . . . Several little nests of sand are scattered here and there through the body of the clay, which, despite the presence of these and the gravel and layer of boulders, is a perfectly unstratified and amorphous mass of clay and angular stones scratched and polished. . . . This is the invariable character of the till or boulder clay" (*op. cit.*, pp. 18, 19).

In the third edition of the same work this paragraph has been cut out, and we are told that "although the till of Scotland is a tumultuous and amorphous mass, yet it is not entirely devoid of structure. When we view a good section of it at some little distance we may occasionally notice a kind of stratification, the stones appearing to show a rude arrangement in roughly parallel lines, which are sometimes horizontal, but more frequently oblique, diagonal or curved and even involved. . . . Now and again we may detect similarly arranged layers of less stony clay, alternating with bands more abundantly crowded with stones and boulders. The several layers are not sharply differentiated; there is usually a more or less gradual passage from the one into the other, while not unfrequently individual boulders project from an underlying into an overlying layer. Occasionally this rude bedding is marked by difference of colour and sometimes by distinct lines of stones and boulders. . . . A closer inspection of the till sometimes reveals the presence of a kind of lamination, the clay being arranged in very thin, undulating layers, which are often puckered and even contorted, reminding one somewhat of the foliated structure of a gnarled gneiss or mica schist" (*op. cit.*, pp. 14, 15).

Again we read in the same third edition: "Till is not wholly devoid of structure. Sometimes a rude stratification appears, marked by lines or layers of boulders, etc., by alternations of very stony with less stony layers of clay, by a kind of lamination, by the local arrangement of the stones, etc., in the line of their longer axis, and by the occurrence

of striated pavements. The 'stratification' is sometimes horizontal, but more usually diagonal, and often curved and even contorted. Nests and irregular patches and lenticular layers, and thick beds of water-arranged materials, are not infrequently enclosed in till" (*ibid.*, pp. 23, 24).

These descriptions are assuredly quite inconsistent with any theory by which the Scotch till has been laid down by ice whose solid foot would pound into slush all laminæ and traces of stratification. But Mr. Geikie is not daunted. Not only does he reconcile himself to explaining the Scotch till with its more obscure traces of lamination with the spoor of the ice monster, but he gaily invokes it to explain the much more distinctly laminated beds of England, and, in fact, he is more or less bound to take this line, for as we shall show in a later page the heterogeneous beds pass very frequently indeed into those that are stratified and laminated. I will here quote a single graphic instance from the other side of the Atlantic:—

Mr. R. S. Salisbury says: "Broadly speaking, the drift falls into two great classes: (1) The stratified or assorted drift, and (2) the unstratified or unassorted drift. . . . These two general classes of drift are so intimately related to one another that it is sometimes very difficult to draw lines of definition between them. It frequently happens that the one lies upon the other, and not rarely they alternate with each other several times between the surface and the bottom of the drift; . . . any form of drift may grade into others; . . . it is often extremely difficult to make sharp distinctions between them in the field; . . . it often becomes a matter of extreme difficulty to draw a line, separating till on the one hand from stratified sand and gravel on the other; often no such line can be drawn without doing violence to the parts. . . . The stratified drift may overlie the unstratified or *vice versâ* to any depth. One foot of stratified sand may overlie many feet of till, or the reverse may be the case. . . . The thickness of the surface layer in such cases is often very variable; . . . the thickening and thinning follows no law" (*Amer. Rep. State Geol. of New Jersey* for 1891, p. 101, etc.).

Speaking of the purple clays between Bridlington and Filey, Mr. Carter says: "At intervals instances of rough stratifica-

tion are seen in these clay deposits, at one point there being a considerable mass of well-laminated muds" (*ibid.*, p. 422).

Mr. R. M. Deeley says: "The most chalky boulder clay is generally a true ground moraine, but in some places, such as Chellaston, Melton Mowbray, Market Bosworth, Abbots Bromley, etc., it presents indubitable signs of aqueous action" (Pleistocene Succession in the Trent Basin, *Quart. Journ. Geol. Soc.*, 1886, p. 455).

It was perhaps this versatility in the beds which led to Mr. Geikie importing his land-ice monster to explain the contorted drift of England. Here, again, he apparently changed his views between his second and third edition. He has at all events changed his text. In the former edition he tells us that the Cromer till differs considerably from the Scotch till, that it is not infrequently laminated and includes much fewer stones, which are, for the most part, rounded and small. "Towards the top," he says, "the clay becomes distinctly bedded, and seems then to interosculate with, and to shade up into, the overlying contorted drift; at other times, however, the line of separation between the two deposits is sharply defined; lower down the traces of bedding become indistinct." Of the contorted drift he says: "The whole series is laminated and well bedded, save here and there where the brick-earth seems to be amorphous. The drift displays wonderful contorted bedding, the contortions often affecting the whole thickness of the deposit, and even disturbing the underlying boulder clay."

In his third edition Mr. Geikie tells us that while formerly holding the contorted drift to have been caused by icebergs, he has been converted by Mr. Clement Reid to the view that it had been caused by the pressure of an ice-sheet, of which he considered the underlying till as the ground moraine of an ice-sheet, and he quotes approvingly Mr. Reid's statement that the strike of the larger folds in the contorted drift on the coast points to a force acting from the north-east (p. 339). It is literally astounding that any one who has seen the curves in which the beds in question are arranged should attribute their formation to the action of great unyielding masses of ice. Mr. Geikie goes farther and says more explicitly that the traces of lamination shown by the drift of East Anglia

have nothing to do with aqueous sedimentation, and are the result of shearing and fluidal movement under compression!!! (*Ibid.*, p. 340.)

This extraordinary notion is propounded to explain the phenomena so familiar to us in the cliffs on the Norfolk coast, and on that of the Danish island of Moen, etc., where, so far as we know, the same beds which are elsewhere horizontal and regular "suddenly and locally become twisted and contorted about in the most extraordinary fashion".

Let us now examine these contorted beds more narrowly. They are widely distributed and Darwin gives a drawing of some layers of mud inclined at an angle of 65° and forming a regular saddle at Gregory Bay in the east part of the Straits of Magellan. Many of the layers there lose themselves in the sands in the most curious convolutions. Another bed is bent back on itself. The subordinate layers within this bed are themselves puckered up into a series of urn-shaped curves (*Geol. Trans.*, 2nd series, vi., pp. 417-423).

Lyell says of those in Norfolk: "In no portion of Great Britain are there evidences of more complicated disturbances of a modern date than in the mud cliffs of Norfolk, or disturbances more difficult to explain. In many parts of the cliffs the beds are twisted and contorted into every possible curvature, and the replications are in some instances so numerous that a bed may be intersected three times in the same vertical well or boring. Occasionally the beds are vertical, sometimes they present concentric crusts enveloping a nucleus of sand, gravel or chalk, occasionally more than twenty feet in diameter." In one instance a nucleus of blue clay was successively surrounded by layers of white sand, yellow sand, striped loam and clay, laminated blue clay, and in another case he counted thirty distinct strata enveloping a nucleus of sand. "These strange bendings, twistings and other irregularities are not continuous though characteristic of the greater portion of the cliffs, but are in some cases limited to a small area, both in vertical height and lateral extension. Occasionally they range from the top to the bottom of the cliff, but not infrequently beds thus disturbed rest upon others perfectly horizontal." Lyell attributes these phenomena hesitatingly to ice, but is evidently much

puzzled (*ibid.*, pp. 171-179). Later champions of ice have not been so modest.

The ice monster, whose tread in many places was hypothetically so gentle that it must have passed over hundreds of square miles of soft beds without denuding or displacing them, is thus supposed to have suddenly concentrated its vigour upon a small strip of coast in Norfolk, and another small strip in Denmark, leaving the surrounding districts unaffected. Not only so, but it went so far in these instances as to crumple up and tear asunder the solid beds of chalk below, and even to do this when they were protected from its immediate action by deep cushions of soft sand and clay!!! Where is there any empirical evidence in the work of modern glaciers which would justify us in supposing that ice, even when moving over solid rocks with slow, uniform pressure, will crumple and tear and distort them in this way? How, again, would it be possible for an ice-sheet to contort beds into the sinuous and re-entering curves, the huge twisted S-shaped swirls that the laminæ and striæ adopt in the Norfolk cliffs? How is it physically possible that a solid mass of ice moving and pressing in a uniform direction could perform these dynamical evolutions? Surely the thing has only to be mentioned to refute itself. If we turn from an ice-sheet to icebergs, are we any better off? Icebergs presuppose a general continued submergence, of which, as we have so often said, we have no adequate traces. Icebergs anchored and only partially buoyed up by the water no doubt scrape the bottom and score it considerably when moved or rocked by the wind or the tide, but where have we any evidence that they can smash up and twist solid strata in the way the chalk has been smashed and twisted here? How could they do it with a thick buffer of sand and clay between, and what iceberg, as has been asked elsewhere, could create contortions in 200 feet of strata? If they could, why should they not have done it over the much more highly glaciated regions farther north, and have reserved themselves for these limited areas? How could icebergs arrange sands in beautiful curves and twists? If they pounded the beds they would churn them into homogeneous mud and not leave these delicate contortions. How, again, can we understand either icebergs or an ice-sheet, or any more or less solid mass acting from

above, twisting and curling the drift and leaving the soft beds below ranged in beautiful horizontal fashion and undisturbed save at certain points? West of Cromer they can be followed for miles lying like interlaced snakes upon perfectly undisturbed laminated beds. This was repeatedly pointed out to me by Mr. Blake, and it is surely a very remarkable fact, and one which, as he, a great authority on these East Anglian beds, said to me emphatically, seemed inconsistent with any pounding action from above.

There is again the theory of the Rev. O. Fisher that the contortions which go down to the solid strata below are due to crushing caused by the piling up of great masses of material on certain portions while neighbouring places are clear from it. In regard to this modification of the iceberg theory, Mr. Reid's objection seems fatal. He says: "This cause seems inadequate to the formation of contortions on so large a scale. It is doubtful whether anything less than a mountain piled on the surface at Trimingham would be sufficient for the contortion of 200 feet of underlying strata; and even that would scarcely account for the inversion of the chalk." In addition to this, why should such an effect from such a cause be so local, and why, over large spaces, should the contortions go down to the base of the contorted drift and then completely cease, and the soft laminated clays and sand beds below be undisturbed? Lastly, it has been suggested that as in Siberia the ground here was once hard frozen, and that layers or masses of ice were intercalated with beds of sand and clay, and that the thawing of these masses of ice caused subsidences, etc., which in turn produced the contortions. This view is equally with the others astounding to ingenuous students uncommitted to the official view of the Olympians. How could the melting of pockets and strata of ice smash and crumple up the chalk beneath? How, by such a method, could the sand and gravel be twisted into the shape of a great letter S standing upright, and into a second S close by standing obliquely, and into the various snake-like contortions we find in these cliffs?

Again, how is it possible for any one who has with any care examined the convolutions and twists to believe that they are the remains of once continuous horizontal layers (as seems to be the view of some), and that the curves and twists were super-

imposed upon these layers by subsequent disturbance? Very many of the figures are absolutely detached and as substantive and independent as so many letters drawn on a blackboard. There is no question of piecing together broken links. They form no links whatever of any chain, while in the case of some of the elaborately convoluted ribbons, if they were stretched out there would not be room to hold them in the space at our command. We must also remember that we get these curves and twists whether we view the beds as displayed on the face of the cliffs or in sections cut at right angles to them, as Mr. Mellard Reade has shown, which again militates completely against their being the results of lateral pressure caused by the impinging of any solid mass, iceberg or otherwise. Let us, however, turn from the school of glacialists which never seems to verify the possibility of its premises, to a glacialist who, in this instance, is more consistent. What does Mr. Carvell Lewis, a great glacial champion, say of these beds? "I examined," he says, "the cliffs from Sidestrand to Runton, six miles in length, but found no till and no evidence whatever of glaciation;" and again, "I am astonished at the many statements and numerous papers concerning the Cromer till, which has no resemblance to till at all. . . . There is no evidence of glaciation. The contortions were probably made at the time of the elevation of the coast" (*op. cit.*, p. 339). Again he says: "It is a mistake to call any part of the contorted beds at Cromer Cromer till, as there is no true till there" (*ibid.*, p. 60). The same view has, I believe, been strenuously maintained by my acute friend, Prof. Gregory.

Let us now turn from the contortions and laminations to another widespread feature of the southern drifts, namely, the fact that they are in so very many cases separated into clays on the one hand and sands and gravels on the other. Surely this sifting is absolutely inconsistent with the work of an ice foot which makes heterogeneous slush and muck and cannot sift clays from sands.

The fact of the separation of the clays and the sands into distinct beds almost everywhere seems to me to remove them entirely from all kinds of sub-glacial deposits. If a glacier is working over a bed of clay slate it will, no doubt, when it

erodes it, manufacture a deposit of clay; if it is similarly working over and eroding a bed of sandstone it will probably make a bed of sand, and if its bed be composed of both kinds of rock the product of the two will be mixed into a jumble, into what Americans call "muck," such as we see in the moraine mounds of the Alps. The clay and the sand will be mingled together.

By what possible process a mass of ice could separate the two ingredients when mixed into beds of clay and sand respectively is quite unknown to me. How, again, if the ice were moving over the same ground it could deposit at one time clean clay and at another clean sand seems quite incomprehensible. Such sifting can only have been the handiwork of water (see *Glacial Nightmare*, pp. 782, 783).

If it was water that sifted the ingredients in question, then the clay no less than the sand is a water deposit, and if so, what comes of all the arguments in favour of boulder clay being *a moraine profonde in situ?* If both were deposited and sifted by water, it seems past all belief to appeal to subglacial streams, which could only wash the contents of their channels and not the whole mass postulated as underlying the ice-sheet, and even if this difficulty were removed and the sifting of the materials were accomplished, how could streams proceeding from the same ice-sheets lay down at one time sand and at another clay? The reasonable explanation of such deposits as we find in the drift is, that both the clays and sands had already been made and sifted in former ages, and that they were taken up by vast masses of water, coming at one time from one direction and at another from another, during a period of abnormal terrestrial dislocation, and carrying away the beds they encountered, at one time clay and at another sand, just as we find the shifting tide laying down successive beds of gravel, sand and clay.

The existence of the sifted beds has not apparently embarrassed the foster-fathers of the glacial theory. They have gaily passed it over in regard to the clays, while appealing to water as the agent which has laid down the sands. It has not occurred to them that the existence of these beds of clay, which have been washed clean of sand, is as much an evidence of their having been laid down by water as is that of the sands

with their laminations and cross bedding. The glacialists have, nevertheless, invented a quite arbitrary separation of the two sets of beds, and treated the clays as the products of land-ice and the sands as aqueous. The only product of land-ice we know in nature is neither sifted clay nor sand, but heterogeneous "muck".

The fact that these intervening sands are almost invariably laminated and stratified and cross-bedded makes, in fact, the conclusion that they are subaqueous inevitable, and the glacialists have accordingly had to account for a water deposit in an age of omnipresent ice when the clays are supposed to have been laid down. Their escape from the dilemma has necessitated their attributing the deposition of the sands to one or more interglacial temperate periods, during which the rapid melting of the ice is supposed to have caused extraordinary floods. One of these periods has been named the Champlain Period in America, while the sands have generally been known in England as the Middle Sands or the Interglacial Sands. I have already shown that the organic contents of these sands give no countenance to the notion of such interglacial mild periods, the mollusca in the sands showing, if anything, rather a colder than a warmer facies than the mollusca in the clays. I will only now quote one additional case. Mr. J. Marr urges that all the three supposed divisions of the glacial beds contain shells of precisely the same kinds. The shells from Moel Tryfaen are more Arctic than those of the Severn valley drifts. He had found a number of fragments of similar shells to those of the Arctic drift of the Severn valley in an outcrop of clean gravel overlain by sixty feet of boulder clay, between Coddenham and Crowfield in Suffolk (*Geol. Mag.*, i., p. 295).

This shows that the evidence of the shells in no way supports the theory of interglacial mild climates. There is just as great a difficulty in assigning the intercalated sands and gravels themselves apart from their organic contents to intermittent mild conditions. In the first place we have the difficulty in many cases of separating the sands from the clays except by the accidental fact of their being composed of silicious or aluminous materials. Thus, to quote some sound authorities, Mr. Judd in his *Geology of Rutland* says: "Although

some geologists have attempted to show that the great glacial formations composed of clay and sand or gravel respectively belong to perfectly distinct periods and mark different climatal and physical conditions in the middle districts of England, and even entire changes in the disposition of the land and sea of the period; yet nothing can be clearer than that in the area we are more particularly describing the beds of glacial clay, sand and gravel replace one another in the most capricious manner, and are evidently dependent on the action of causes of extremely local character" (*ibid.*, p. 247).

Speaking of some drift beds in the Isle of Man, Mr. Kendall says: "It is important in this section that there is an appearance of bedding in the boulder clay which is roughly conformable to that of the gravels. . . ." The clays and gravels present the appearance of having been worked into one another. The sand and gravel series exhibits the greatest confusion of bedding, but whether the features should be attributed to marine or to stream action is a question which in the present state of our knowledge cannot be positively decided from the appearance of the beds (*Glacial Geol. of Isle of Man*, p. 17).

The middle sands and gravels, says Mr. M. Reade, are supposed by some to represent "interglacial" and by others "milder" conditions, in consequence of the stones being all rounded and unglaciated. "If this be the case, what do the sands and gravels of St. Bees represent? They are even a more distinct deposit, and overlie a lower clay with an eroded surface; but here the conditions are reversed, for the sands and gravels contain in places more and larger blocks than the lower boulder clays of Blackpool, and they are full of contortions and evidence of violent action of some sort" (*Quar. Journ. Geol. Soc.*, 1883, pp. 110, 111). Again he says: "The sands and gravels at St. Bees, which are in great force, are distinguished by contorted bedding, confused aggregation and the great number of included blocks and boulders. The shingle, gravel and sands are intruded or folded into each other. The underlying clay is peculiarly free from stones. If we found geological subdivisions on such grounds, what are we to do with these beds? No one has ever suggested an explanation" (*ibid.*, p. 171).

Messrs. Wood and Harmer have, in an elaborate paper, argued for a period of great denudation between the contorted drift and the so-called middle glacial beds. Mr. Reid and Mr. Jukes-Browne have failed to see any evidence of this in the section between Weyburn and Hasboro'. Woodward says: "I am bound to add that nowhere in the neighbourhood have I been able to detect evidence of great denudation between the middle glacial sands and the lower glacial brick-earths, although here and there we find evidence of local erosion. A consideration of all the facts tends to show the intimate connection between the lower boulder clay, contorted drift and middle glacial, as one tolerably continuous series of deposits. . . . Mr. Blake maintained the close connection of these deposits long before my own opinion had become settled" (*Proc. Norwich Geol. Soc.*, 1878-89, p. 57, etc.).

What we have seen to be the case in this behalf in Europe is equally the case on the other side of the Atlantic. Speaking of the drift beds in the Bow River valley, Prof. G. Dawson says: "It is here quite clear that the boulder clay and silts represent a single deposit which took place under varying conditions, and in which the boulder clay forms, broadly speaking, lenticular masses, not persistent and not characteristic of any particular horizon, or coextensive with the region of deposit. The section as a whole, moreover, is that of a series of stratified deposits, in which evidences of tumultuous deposit and obscure bedding occur only in the case of the boulder clay and the underlying gravels" (*Bull. Geol. Soc. Amer.*, vii., p. 55).

All this surely points very clearly to the sands and clays having been the contemporaneous products of varying phases of one movement, and not relics of successive periods.

Again, we have often a difficulty where sands intervene between the clays in finding any features by which to distinguish the clays above the sands from those below them. If these clays, as they hypothetically do, represent great geological periods, separated by other geological periods, they ought to show some signs of variation. It is almost incredible that their features should be so closely repeated. Thus Mr. Geikie, in dividing the Scotch till into two horizons, has the greatest difficulty, as others have had, in finding any feature

by which to discriminate them except his *a priori* premiss, and he admits that it is very difficult to distinguish one from the other. Sometimes, as he allows, the two beds rest directly on each other, and when he separates them by a division, it is by the presence of some fragment of a fresh-water bed of gravel, sand, clay and silt which is intercalated in them, and which he treats as interglacial. I have in chapter xii. shown that in all cases where these drift beds contain organic remains, the latter lie when *in situ* below the drift, and that when they occur in it, it is as boulders of greater or less size.

Mr. De Rance says: "The upper boulder clay resembles, in the whole of Lancashire, from Ulverston to Manchester, the lower boulder clay of the southern part of the *low country* in its physical character, chemical composition, included erratic fragments, and the species of shells of mollusca found with it. Both clays contain more silurian erratics in the north-west, and more carboniferous erratics in the south-east of Lancashire; both are of a dull Indian-red coloured tint, caused by the presence of iron derived partly from the triassic rocks, and partly from the hæmatite deposits of Furness, in which district the upper boulder clay has a deep, almost lurid colour" (*Quar. Journ. Geol. Soc.*, 1870, p. 641).

In the next place, the advocates of an interglacial period to account for the sands are, as I showed in my former volumes, greatly embarrassed by the fact that there is no law, no order of any kind, regulating *the succession* of the beds, sometimes the clays being intercalated between the sands and sometimes the sands between the clays. In my *Glacial Nightmare* (pp. 314-322, 470-472, 843-853) I have discussed at great length the question of interglacial periods as evidenced by the intercalation of successive clays and sands, and have collected the views of some of the most experienced of the writers on the surface beds, showing how impossible it is to arrange these drift beds in any consistent order, and how utterly wanting in a scientific basis is the theory of interglacial climates as evidenced by the sands and clays of the drift. I will now add some other opinions of experienced explorers of the drift beds on this critical question.

First, there is no question which ought, one would think, to be more easy to settle, and upon which there is notwithstand-

ing so much variation of opinion, as the number of glacial and interglacial periods. Mr. J. Geikie postulates an extraordinary number of such changes, which, as Mr. D. Bell says, seem to be utterly disconnected and at variance with each other. Of this Bell quotes a few instances as follows from the *Great Ice Age* :—

"The climate must certainly have become cold and ungenial as the depression continued" (*op. cit.*, p. 312). "Scotland was submerged. . . . The climate had deteriorated" (*ibid.*). "The land gained on the sea until the latter had retreated considerably beyond its present limits. The climate at the same time became more genial" (p. 313) "Submergence next ensued, the climate at the same time passing from temperate to Arctic" (p. 323). "Re-elevation of the land or retreat of the sea, and a gradual amelioration of climate" (p. 325). "Submergence once more ensued . . . and the climate at the same time became colder" (*ibid.*).

Dr. Geikie's latest "scheme," as Mr. Bell says, seems to include at least *five* glacial and *four* interglacial periods, accompanied by submergences and re-elevations to the extent of between 500 and 600 feet (*Great Ice Age*, pp. 323-325, 421, 422; *Fragments of Earth Lore*, pp. 319-321).

From these and other passages which might be quoted it would seem that the author regards elevation as somehow productive of more genial conditions, and submergence as associated with colder and more severe conditions of climate.

Mr. Bell says (surely most justly) that it may be taken as a certain datum, that in itself a high elevation of the land tends to bring about cold conditions, and a depression or submergence milder conditions of climate.

We may here remark that these numerous distinct glacial periods, accompanied by repeated depressions and re-elevations, are not borne out by the observations of geologists in other countries.

"Mr. Geikie admits," says Mr. Bell, "that the majority of geologists on the continent believe that there have been only two glacial epochs, separated by an interglacial epoch of more genial conditions" (*op. cit.*, pp. 448-456). In America we have among most geologists a similar division into two glacial epochs with an intermediate mild period (see Chamberlin in the *Great Ice Age*, p. 773), while others allow really only one

epoch varying in intensity and local effects. I most completely agree with Mr. D. Bell's concluding paragraph on this subject: "While not unaware," he says, "of local difficulties, we respectfully submit that Dr. Geikie's complicated system of so many glacial and interglacial periods and so many submergences and re-emergences, from 500 to 600 feet downwards, with their varying and apparently inscrutable effects upon climate, tends, on the whole, to increase these difficulties and should therefore *be reformed altogether*" (*Geol. Mag.*, 1895, p. 16).

While Prof. Geikie is more or less consistent, since he logically faces the issue and bravely postulates a fresh glacial or interglacial period to explain every intercalated bed,[1] others have been less logical and more modest, and have appealed to a more limited number of changes.

Dakyns says: "Too much importance has been attached to cases of partial unconformity in the so-called glacial beds, as if they marked a great difference of age. Thus many writers have recognised three boulder clays, whereas in some places there are four, with a similar number of unconformities, as at Dane's Dike and the High Stacks. . . . The break is in the glacial beds. Unconformities (so called) and eroded surfaces, instead of indicating great breaks in the series, are, in nine cases out of ten, merely local and contemporaneous. . . . It is all one great boulder clay formation. Too much faith has also been placed in different lithological conditions and colour. These are local differences, . . . all blue rocks are apt to turn red by chemical action" (*Proc. Geol. and Pol. Soc.*, York, vii., pp. 126-128).

Messrs. Wood and Harmer classed the drift of East Anglia thus: (1) Plateau gravel = doubtfully glacial; (2) chalky boulder clay = upper glacial, sands and gravels = middle glacial; (3) contorted drift, Cromer till, Bure valley beds = lower glacial. Mr. Gunn's arrangement is quite different, *viz.*, (1) Upper boulder clay; (2) stratified clays with sands and gravels; (3) lower boulder clay or till.

On these classifications Mr. Horace Woodward comments.

[1] Mr. C. Lewis says that according to Mr. Geikie there ought in fact to be evidence at Cheetham Hill, near Manchester, of five glacial periods (*op. cit.*, p. 283).

Mr. Gunn's upper boulder clay is the equivalent of the chalky boulder clay, his lower boulder clay is the Cromer till of the coast section. But the stratified clays with sands and gravel "embrace both the contorted drift and middle glacial of Messrs. Wood and Harmer". He has classed as one beds which they separate into two, marked by a period of great denudation. But on the Cromer coast near Mundesley and other places, it is often difficult to draw a line between the Cromer till or lower boulder clay and the contorted drift. They merge one into the other. At Corton there is a stony loam, and around Mousehold there is brick-earth, called contorted drift by Messrs. Wood and Harmer and lower boulder clay by Mr. Gunn. Hence we see there is not always a definite distinction between lower boulder clay and contorted drift. Again, he elsewhere speaks of the same classification of the drift beds by Wood and Harmer.

"I have been at a loss to find the persistence of any of the four divisions. . . . Beds have frequent and often very abrupt changes in lithological character. . . . In many places the lower and upper boulder clay is identical. Sometimes contorted drift passes into the great chalky boulder clay" (*Geol. of Country round Fakenham*, etc.).

Kendall, speaking of the Lancashire drifts, says: "The glacial deposits have been classified by Prof. E. Hull (*Proc. Lit. and Phil. Soc.*, March, 1865, p. 449) into three groups, *viz.*, (1) Lower boulder clay; (2) middle sands and gravels; (3) upper boulder clay. This classification has been adopted by the officers of H.M.'s Geological Survey, and by many independent geologists, though not a few of the closest students of the glaciation of the district have found themselves opposed to it. . . . The lower and upper boulder clays are assigned respectively to the period of major and minor glaciation, while the middle sands are allocated to the interglacial period.

"From this classification," says Mr. Kendall, "and from the theoretical conclusions drawn from it, I alike dissent. The stratigraphical evidence cannot be considered as conclusive because of its extreme flexibility. For example, suppose in a given section a mass of sand to be seen interposed between two beds of clay, it will be claimed that the series is complete. Suppose a bed of sand to repose *upon* clay, here it will be

said that the upper clay has been removed by denudation. Should the deposits above the solid rock consist of sand covered by clay, it will be interpreted to mean that the lower boulder clay had been removed by denudation before the deposition of the sand. Such arguments as these are at one and the same time unanswerable and quite inconclusive.

"The *lithological* character might be expected to yield a safer guide, and, in truth, it is of crucial importance to determine whether or not there is a radical and essential difference between the upper and the lower boulder clay. It is indeed a vital necessity to the tripartite scheme that there should be some intrinsic features, either of composition or of contents, by which the clays may be differentiated.

"These two deposits of clay are, however, indistinguishable. It has been supposed that the lower boulder clay is the harder and more stony of the two; but against this I may cite as one among the many cases which have come under my own observation an instance at Heaton Mersey. Here a bed of clay rests directly on the triassic sandstone, while it is overlaid by typical 'middle sands'. The clay is through almost its entire thickness finely laminated and crumpled; singularly few stones are embedded in it; it is very soft and contains many shell fragments.

"On the other hand, a thick bed of clay which rests upon the 'middle sands' near at hand is hard and tough, and contains many well-scratched stones. One such example as this is sufficient to dispose of any lithological test for lower boulder clay, and my own observations show that the beds vary between the uttermost extremes within a very short distance.

"Moreover, whenever sections of any considerable length are opened, they rarely fail to exhibit lenticular patches of sand and gravel interposed between beds of boulder clay, and in many cases there are two or three beds of sand in one vertical section. These beds of sand occasionally attain very large dimensions—for example, in the neighbourhood of Preston they are traceable for several miles, and again in the neighbourhood of Stockport. It must be noted, however, that the outcrops are singularly intermittent, and that the sands vanish and reappear in a very erratic manner, while partings of stony clay occur from time to time.

"It appears to me that the occurrence of two or three superimposed beds of clay, with partings of sand and gravel of greater or less extent, do not mark actual subdivisions of the glacial epoch, but local phases of deposition, so that sands might be deposited side by side with (so-called) 'upper' or 'lower' boulder clay. In other words, I think that the varieties in the deposit are a testimony to differences not of age, but solely of conditions. . . . While the stratification is of so inconstant a character as to invalidate the hypothesis that there is any definite bed of sand or other material representing an interglacial period in the Lancashire and Cheshire area, the palæontological evidence is equally unsatisfactory" (Carvell Lewis, *The Glacial Geology of Great Britain*, pp. 411-413). This palæontological evidence I have discussed in a previous chapter (see vol. ii., p. 114, etc.).

I agree very much with the general conclusions of Mr. Kendall here cited; what I cannot agree with is his opinion that these sands were deposited by sub-glacial streams. Such a conclusion seems absolutely beyond the range of possibility in regard to beds distributed, as these sands are, not in streams, or ribbons, or bands, but in almost continuous sheets. But to continue. A few sections at the foot of Stainmoor in the Vale of Eden, says Mr. Goodchild, show that locally a threefold division of the drift obtains, yet he adds farther on: "It is nearly impossible to make out any definite order of succession in the drifts in the lower parts of the valley; the few sections seen show plainly enough that masses of sand and gravel pass into and are interwoven with clay drifts in such a way as to defy any attempt at separation over large areas". An inspection of the remarkable series of sections from the cuttings of the Settle and Carlisle Railway given in Mr. Goodchild's paper, showing the intimate interweaving of stratified sands and gravels, and laminated clays with till and boulder clay, indicates the utter impossibility of referring the former to a distinct period (*Geol.*, May, 1891, pp. 7-8).

Mr. Mellard Reade, speaking of the two boulder clays which have been postulated at Blackpool in Lancashire, says: "If we could see a section farther inland, it is highly probable that these two clays would coalesce and shade into each other in places, as indeed is shown to be the case in Mr.

Binney's section made twenty years earlier. Mr. Binney himself says that his bed number two, a brownish-coloured clay containing stones, . . . is often replaced by stratified beds of sand and gravel." If we confine ourselves to Blackpool it may be perfectly natural to speak of a threefold division; but the moment we attempt to apply the same classification elsewhere, we are met by insurmountable difficulties.

If we are to classify by superposition, the clay at St. Bees will be lower boulder clay; but, unfortunately, it does not correspond in any other way with that of Blackpool, being nearly stoneless. Again, at Rampside, we have two sections side by side on the same horizon, one being full of glaciated stones, and answering to the same description of "lower boulder clay," the other corresponding more with what is called "upper boulder clay," but in it are the arched beds of sand. There is no evidence whatever that one clay is laid over the other.

Mr. Crosskey was also strongly opposed to the notion that the drift deposits were susceptible of a threefold division. He refers to the sections of wells published by J. W. Gray, in his paper on the geology of Stockport, as showing clearly that no threefold division of the drift will suffice (Intr. to Carvell Lewis' *Glac. Geol. of Great Britain*, xli.). In his *Building of the British Isles*, Mr. A. J. Jukes-Browne writes that the most recent detailed observations on the later glacial series of Yorkshire, Lincolnshire, Cheshire and Lancashire have shown that the supposed division of those beds into an upper and lower boulder clay, separated by a set of marine sands and gravels, is incorrect, the sands and gravels being only lenticular beds and occurring at various horizons in the series.

I do not know how the case here made out against the notion of interglacial periods is to be answered, and in fact the champions of that notion have become much fewer than they were. When Croll startled the world and carried men along with him by his impetuous and brilliant book, *Climate and Time*, he made converts wholesale to the notion of interglacial periods. The notion was a necessity of his argument as it was of that of Sir R. Ball, and in fact of all theories of an ice age based directly or indirectly on astronomical grounds,

and involving a recurrence of ice ages at intervals as a part of the scheme. "It is to Dr. Croll," says Ball, "must be ascribed the credit of having pointed out that the remarkable succession of ice ages was a necessary consequence of the astronomical theory of their origin." "Again," says Ball, "we are entitled to infer as a necessary consequence of the astronomical theory, that throughout the whole period spoken of as geological time, ice ages have from time to time recurred according to a law of succession prescribed by astronomical conditions" (*Cause of an Ice Age*, 2nd ed., pp. 140, 156).

When we ask for some proof, however, Dr. Ball as usual gives us a mere *obiter dictum* on the subject. "Geology," he says, "affords us distinct traces of the repetition of glacial phenomena" (*ibid.*, p. 140). To this bare statement the great majority of living geologists would entirely demur. The champions of interglacial climates are, as I have said, a shrinking number. The *raison d'être* of the theory was, in effect, the necessity imposed upon the geologists by the champions of the astronomical theory. Now that that theory has been shown to be quite invalid as a mediate or an immediate cause of ice ages, the necessity in question no longer exists, and geologists have been able to survey the facts of their science with a more neutral eye. The result has been a steady decay in the number of the champions of interglacial climates, especially in America and among non-official geologists in Britain. Professor Geikie and the geological surveyors, in Britain, and Penck and his followers in Germany, still hold to it, I believe, as some of them hold on hesitatingly to Croll's theory of an ice age, of which it is a mere corollary. For the rest, the facts, of which only samples have been here given, seem to overwhelmingly proclaim the futility of the notion.

The view held by the more recent glacial geologists on the subject may be summed up, in fact, in the words of an ardent glacialist. "My observations convince me," says Lewis, "that the glacial period, instead of being a complex series of glacial advances and retreats, and of aqueous submergences and re-elevations accompanied by recurrent cold and warm periods, was in reality as simple as one of the preceding tertiary periods" (*op. cit.*, p. 71).

Let us proceed, however.

The intercalated beds referred to above, and attributed to interglacial periods, are very often not continuous, but mere patches. Mr. Geikie himself describes how patches of clay are sometimes found intercalated among the stratified gravels, and tells us he was told by the workmen in a pit that such a patch was what they called a "leg," and was connected with the stuff at the top, from which, they said, several such "legs" had come and had given them much trouble in the working. Similar appearances are occasionally found, says Mr. Geikie, in connection with the intercalated and subjacent beds of Scotch till. Mr. Geikie explains these as tongues of till squeezed through and between the yielding deposits, over which the sub-glacial mass was rolled and pressed (*Great Ice Age*, p. 362). How the process here appealed to could be mechanically carried out, except in the perfervid Scotch imagination of the most ingenuous professor, I do not know. Considering that these same ice-sheets are supposed to have travelled over widely spread laminated beds without disturbing them, the process does seem to me miraculous.

I am bound to say again that the alleged transport of stratified lumps of what are apparently tertiary beds of soft strata with their stratification intact by ice-sheets seems just as miraculous. I have previously described at length several instances of these transported masses of soft strata with their layers intact (vol. ii., pp. 223-226). It is not only in the true boulder clays that these blocks of soft tertiary strata occur in this way.

Mr. O. Fisher similarly refers to considerable portions of "trail" as transferred *en bloc*, like a huge boulder (*Geol. Mag.*, viii., pp. 66-70).

How is it possible to attribute the detachment from its matrix and subsequent transport of material like this to ice? To show the desperate straits to which the champions of ice have, in fact, been driven by this fact, I will quote, without criticising, Mr. Lamplugh's condensed statement of the facts with his conclusions, which I cannot help describing as fantastic. He says of the beds near Bridlington: "The tendency of the basement clay to include patches of other deposits I regard as one of its chief characteristics. . . . The inclusions are not always beds of glacial age, but are sometimes derived from the secondary formation of masses of

liassic shale and neocomian clay occurring, the former, among other places, in Filey Bay and at Bridlington, and the latter at several places on Flamborough Head. The conditions under which these masses of the older formations are found, exactly parallel the occurrence of the shell-bearing patches, the original bedding being sheared and shaken, but not obliterated, the fossils being preserved, though generally fractured, and each mass forming an isolated boulder in the clay. Some of them, we know, must have travelled no inconsiderable distance, as, for instance, the patches of lower lias at Filey and Bridlington, which cannot have journeyed for less than several miles, even if we allow that strata of that age may crop out in the bed of the North Sea nearer than on the coast line" (*Proc. York Geol. and Pol. Soc.*, 1889, pp. 282, 283). These being the facts, Mr. Lamplugh thus applies his land-ice theory to explain them: "When I try to conceive how the soft shales and incoherent sandy beds came to be taken up from the sea-bottom and transported, as most undoubtedly they have been, for long distances, difficulties arise, and I fail to reach a safe conclusion. Were the beds littoral, one might suppose that floating ice from the ice-foot assisted in the work by removing and carrying portions of beach deposits to places in which they were afterwards overtaken and over-ridden by the steady, forward, creeping land-ice, but many of the shells indicate comparatively deep water, and may have come from almost anywhere in the bed of the southern part of the North Sea." He then adds the following astounding statement: "*The evidence is distinctly in favour of the view that these incoherent masses became actually imbedded in the ice, and rose with it to the higher levels, just as we know the solid boulders have sometimes done; but how this was effected I cannot make out, unless we may imagine that 'anchor ice' had formed, and affixed itself to the sea-bottom before the encroachment of the land-ice with which it afterwards combined*" ! ! ! (*ibid.*, p. 289) Again, I would ask, is this inductive science?

Let us proceed, however.

The division between the supposed horizons treated as marking glacial conditions is not always a bed of sand, peat, etc. We are frequently told that it is sometimes marked by a

plane of erosion. Mr. Jukes-Browne contests the existence of such interglacial erosion in the drift beds of Norfolk (where it had been claimed as a phenomenon), in which he quite agreed with Mr. C. Reid and Mr. Blake. He urges that the apparent hollows in the contorted drift are not channels of erosion, but large contortions affecting both the gravels and the loams alike, and there is no evidence of unconformability between the contorted drift and the so-called middle glacial beds, and concludes that the lower part of the glacial series in Norfolk contains thick beds of stiff, sandy clay or loam which have a lenticular development on a large scale, both sands and loams being affected by the same contortions; that inland the loam is often altogether replaced by sand, and that both form parts of one indivisible whole. "I look upon the contorted drift as merely a local facies of the lower part of the series elsewhere called middle glacial, just as in parts of Suffolk beds of laminated loam and brick-earth are enclosed in the upper part of the same series" (*Proc. Norwich Geol. Soc.*, pp. 171, 172).

Sometimes the boulder clay has very persistent layers or lines or strings of stratified matter running through it which cannot be treated as a special horizon.

Thus the purple clay of Yorkshire is a dark brownish-purple clay containing a great variety of boulders and a few shell fragments. "The chief point of interest in this section (*i.e.*, that south of the harbour at Bridlington) lies in the existence of a well-stratified portion which occurs along one horizon, and thus forms a band running thread-like through the section. This stratified band varies in thickness from a few inches to three feet; it does not differ much from the rest of the clay except in being bedded, but is rather more earthy (which causes it to weather faster), and also in containing a sprinkling of small chalk pebbles, and these are rare in the chalk below it, and not plentiful above. It contains scratched blocks like the rest of the boulder clay, but flat pebbles are nearly always laid horizontally. The bedding is sometimes very distinct, and almost fine enough to be called lamination; at others it is almost or quite lost, though the state of the cliff has something to do with this, as it is after the washing of a heavy sea that the bedding is best brought out. . . . Its

junction with the boulder clay below is sharply defined, but upwards it is sometimes vague" (*Proc. York Geol. and Pol. Soc.*, 1882, p. 33). In regard to this stratification the author just quoted says: "It must not be forgotten by the glacial theorist. At first glance the stratification might be supposed to occur at random throughout the mass of the clay, owing to the constant change in the height and position of the band in a cliff-face, always in some degree marked by landslips. . . . The material of which it is composed differs so slightly from the clay above and below, that it has almost certainly been derived from the same source; yet there can be no doubt that this material has been sorted and deposited in water, for sometimes streaks of sand make their appearance along every bedding plane. It cannot, therefore, be directly the product of land-ice; neither is its position nor composition such as I should expect from a sub-glacial river nor from icebergs. It looks to me as if deposited in open water of no great depth, with no ice excepting light floes" (*ibid.*, pp. 37, 38).

Mr. D. Mackintosh again says, speaking of the drift deposits about the Dee: "There is a distinct and undulating line of junction between the upper and the lower boulder clays wherever the middle sands are absent, and between these sands and the upper boulder clay 'there is almost universally a clean and straight or undulating line of junction, without the slightest commingling of materials'". He thinks that this proves that the upper clay could not have been brought by land-ice, "*because land-ice could not have pushed its* moraine profonde *for scores of miles over an extensive deposit* of yielding sand and gravel without confusedly mixing up the two formations" (*Quar. Journ. Geol. Soc.*, 1877, p. 730, etc.).

Mackintosh also calls attention to a striking fact to which Prof. Hull, Mr. De Rance and others have referred, namely, the evidence in certain places of a great leap from a so-called interglacial sand and gravel period when no glaciated stones were floated by the ice over the submerged plains and when scarcely any clay was deposited to a period when clay and intensely glaciated stones began to accumulate. "What," he says, "could have obliterated all signs of the interval during which the change from non-glacial to glacial conditions took

place? The revolution in temperature may certainly have occurred during an intervening period when dry land existed. But as there are no traces of a land surface, so far as I have seen, between the middle sand and upper clay, I would suggest that the commencement of the upper clay submergence may have been sudden so as to generate an earthquake-wave capable of accomplishing the sweeping denudation of the sand which is so forcibly suggested by the clean and persistent line of separation above described" (*Quart. Journ. Geol. Soc.*, 1877, pp. 735-736). Can anything be more touching than these straits to which the glacial champions are driven in turning the flank of the very difficult position they have made for themselves?

The mixture of stones and of clays and other débris, which although found together have come from different directions, is another difficulty presented by so-called glacial deposits which seems insurmountable by an appeal to land-ice. In some cases Mr. Geikie refers to these debatable lands where a medley of foreign materials meet, but he does not explain them, nor does he tell us how masses of solid ice could struggle with each other over the same ground. It is not a question of ice coming down converging valleys and uniting and mixing its load in a common trough. It is ice-sheets apparently moving in opposite directions over the same ground without clearing off each other's trail. Thus he says: "The right of possession to the tract of country that lies between Cambuslang and Lesmahagow seems frequently to have been disputed by the rival ice streams, the rocks of that area having been sometimes striated by ice that moved from the north, and sometimes again by the confluent masses that flowed from the southern uplands. We find also an intermingling of stones, fragments of mica-schist and gneiss from the Highland mountains occurring now and again as far south as Lesmahagow, while stones, apparently derived from the high grounds to the south of that place, appear here and there in the till as far north as Stonehouse. A similar intermingling of stones from the north and south is seen in the till of the valley of the Esk, near the Moorfoot Hills in Edinburghshire" (*Great Ice Age*, 3rd ed., pp. 77-78). I described at considerable length several instances of a similar mixture of

débris coming from various directions, and now found incongruously together in the drifts of the Baltic countries and elsewhere, in my former work (*Glacial Nightmare*, pp. 730-732), and argued that it was quite inconsistent with land-ice action, whose motion like that of other nearly rigid bodies cannot involve currents in various directions, and I will now only quote an additional instance from Southern England and from a very typical so-called glacial deposit, namely, the chalky boulder clay. The clayey matrix of this deposit has been chiefly derived from the denuded hollow of the Fenlands, while the included stones have come from various directions.

"It must be remembered that this clayey matrix only exists, *in situ*, in the lowest hollows, where the denudation has been greatest. It is thence that the Kimeridge and Oxford clays must have come to form the mass of the chalky clay, and been thence distributed in various directions. It is in the low grounds of the Fen country that the churning and mixing of the materials must have been carried out, and it is thence the clay must afterwards have been sporadically spread out and scattered. How are we by any stretch of the imagination to realise an ice-sheet, formed in the deep hollow of the Fenlands, collecting together from the four winds of heaven materials for the clay, working and mixing them up in the deepest part of the area where it occurs, and then distributing it in various directions, *always* moving uphill from the trough on to the plateau? The kind of reasoning involved is assuredly going back to the dark ages of science, and getting away from induction altogether."

I printed this paragraph some years ago in the *Geological Magazine*. Mr. C. Lewis puts a similar case in other words. In regard to the boulder clay and other drift deposits of Central England, *i.e.*, of Lincolnshire, Norfolk, Suffolk, Cambridge, Nottingham, Stafford, Warwick, etc., he also denies that they are of glacial origin at all, and says: "It would be a curious kind of glacier which could at once carry boulders from the south as well as from the north, from the west as well as from the east, mixing them all in one deposit, and which was able neither to make moraines, nor to striate rock surfaces, nor to smooth projecting crags, nor to make the rocky till" (*op. cit.*, p. 55). Again, he says: "The commingling of the trans-

ported stones in the chalky boulder clay is impossible to explain by the glacial hypothesis" (*ibid.*, p. 59).

It does not seem possible to me that those who have postulated local glaciers thus, sporadically scattering materials containing foreign stones which have come together from all quarters, have ever really measured or thought out the conditions under which they would work at all. Dr. Geikie is equally emphatic. Thus he says: "It may be remembered that in the Midlands of England we encounter numerous erratics which could not have been transported by glacier ice at one and the same time, for they have been carried in different directions. The various lines of transport have crossed each other. The late Mr. Mackintosh pointed to the 'inter-crossing of erratics' in England as showing that these boulders could not have been carried by land-ice" (*Great Ice Age*, pp. 546, 547).

A more remarkable case of a similar kind is mentioned by Mr. Geikie from the Alpine country, where he states that although the old glacier of Valromey is claimed to have flowed down the valley of the Séran which joins the Rhône a little below Culoz for twenty miles, yet erratics which must have travelled up the same valley are found for two-thirds of its length. He falls back upon the notion that these difficulties are to be met by postulating a succession of glacial conditions in which a different direction was given to the ice-flow (*ibid.*).

This conclusion is gaily stated without a particle of mechanical proof of its possibility as is the possibility of the process taken for granted in the next paragraph, according to which when the undercurrent and greater mass of the north branch of the Rhône glacier flowed north-east by Lake Neuchâtel along the flanks of the Jura, the upper strata of the ice streamed across that mountain-chain by various transverse valleys, as Birsthal and Frikthal (*ibid.*).

Another feature of the drift which makes it very difficult to correlate it with true glacial deposits is the fact of the stones in it gradually becoming smaller in size and less in number as we leave their bed-rock. I have referred to this before (*Glacial Nightmare*, p. 699). This is natural in the case of river portage or in the case of portage by some great and

rapid movement of water, but is quite at issue with all we know of land-ice carriage. Mr. Geikie, who admits the facts, does not seem to give enough weight to this difficulty. Following the till," he says, "from the base of the Grampians, where it is crammed with fragments of slate, mica-schist, granite, gneiss, quartzite, etc., down into the basin of the Forth, we find the number of these Highland stones gradually decreasing, until by-and-by they disappear altogether, or are met with only at rare intervals. And the same is the case with the till that stretches northwards from the southern uplands. At first the fragments brought from these uplands are in the majority, but they gradually fall off northwards and finally we cease to meet with them. It is curious also to notice how the stones lose in size as the parent rock is left farther and farther behind the longer the distance travelled" (*op. cit.*, 3rd ed., pp. 77-78). This fact is not limited to the rolled stones, but also applies to the angular erratics and drift—that is to say, to the stones which have been carried, if by ice at all, on the glacier's back, and whose sorting according to their specific gravity seems quite impossible to explain if they were really carried by ice.

Again, Mr. James Geikie says, speaking of the Scotch till: "We may yet occasionally observe that within some given area the larger erratics are arranged with the longer axis of each lying in one and the same direction, and the like has been noticed with regard to the smaller rock fragments and grit of the clay" (*ibid.*, 3rd ed., p. 15). How is this compatible with their having been deposited by land-ice? how could it arrange stones in this symmetrical way? Again, in some cases in boulder clay, as was first pointed out by Mr. Mortimer, of Driffield, the boulders stand perpendicularly, point upwards. He says much of the gravel is found standing on end. Large foreign boulders are often found both in the gravels and clays standing on their ends, and sometimes on their small ends.

Lastly, Mackintosh shows how in the estuary of the Dee, at Dawport, there occur boulders in the so-called lower clay which had evidently fallen from a certain height so as to press the clay violently and cause laminæ to rise up at the sides. Of one of them he gives a figure (*Quart. Journ. Geol. Soc.*, 1877, p. 732).

This will suffice as a criticism of this aspect of the drift. Surely the case here made out is overwhelming, and constitutes a *reductio ad absurdum* of the theory of the ice origin of the drift beds. At every point and in every way when we come to analyse the internal arrangement of the drift beds, we have one accumulated proof that they are quite inconsistent with ice portage. I cannot do better than close with a wail from one of the most courageous champions of that theory.

Mr. Lamplugh confesses that in considering the origin of the East Yorkshire boulder clay the land-ice theory only partially succeeds in explaining the facts. "In the course of my work," he adds, "I have found myself falling into the habit of thinking or speaking of this, or that, as the product of land-ice without having a very clear notion of the properties and capabilities of that curious body.

"In fact, nowadays, an ice-sheet has become a sort of geological comfort, and one can hardly do without it; but somehow, in this neighbourhood, when I have invoked its aid, it has persisted in doing the things it ought not to have done, and leaving undone the things it ought to have done, in so irritating a manner that I have ceased to place much confidence in it. Of course a good glacier under perfect control in a high mountainous country is extremely serviceable and can be held accountable for much; but ice-sheets spreading out over great plains like that now occupied (save for the little corner of Holderness) by the North Sea seem to do much as they like, and to refuse to give any reasonable explanation of their conduct" (*Proc. York Geol. and Pol. Soc.*, viii., p. 253).

It is impossible for me to add anything more crushing than this confession.

CHAPTER XVI.

THE EXTERNAL FEATURES OF THE DRIFT AS A TEST OF THE THEORY OF AN ICE AGE.

"This is the first glacier I have visited, and I brought away the impression that on the whole it was easier to give explanations of glacial phenomena before I had seen ice!"—(Lamplugh, *Glaciation of Vancouver Island*, p. 14.)

In the previous chapter I have tried to analyse the internal structure of the drift beds, and to show that, in every way we can test it, it seems incompatible with the drift having been formed and distributed by ice action. Let us now turn to its external features, upon which I wrote at considerable length in my *Glacial Nightmare*. The question which I propose to ask is, whether the contour and the surface features and general mode of distribution of the drift are consistent with these beds having been laid down by ice.

A glacier, such as we know it, is a mass of ice which moves to some extent *en masse*, so long as its bed has a sufficient slope to give it impetus, but in the main moves as a plastic body by the rolling of one of its layers over another.

When there are overhanging or projecting rocks above its surface, these get disintegrated by the action of frost and weather, and the result is that angular masses of varying size roll down upon the back of the ice. If the slope of the glacier's bed be slight and continuous, the glacier will be a continuous river of ice, without cracks and crevasses, and these angular stones will be carried by the glacier on its back as far as the ice continues to move, retaining their angularity throughout, and remaining angular at the finish.

The amount of this débris on a glacier depends, almost without exception, upon the quantity and rottenness of the exposed rocks on either flank of it.

"Glaciers," says Freshfield, "whose channels are little overhung by rocky slopes have comparatively small moraines. Having fewer tools in their grip in the form of fallen blocks, they will naturally also grind less. The Gul glacier under Ushba is a capital example of a small glacier with enormous moraines derived from the height and rottenness of the crags that encircle its basin" (*Geog. Progress*, 1888, p. 786, note).

Whymper speaks in the same way. He says: "The larger glaciers often have smaller moraines than the small ones, as, for instance, in the two cases of the Gorner and the Z'mutt glacier, the former is much larger, while the moraines are incomparably smaller. The same follows from a comparison of the moraines of the Miage with those of the Glacier d'Argentière" (*Scrambles in the Alps*, p. 243, etc.).

The débris above mentioned having rolled down on to the ice is not distributed uniformly. It forms first a double row of black rubbish flanking the glacier on either side, and, secondly, a certain sprinkling of rocks, etc., which have rolled farther and have reached the more central parts of the glacier's back. The former double rows of débris which travel down with the glacier are what are known as lateral moraines, and they often consist entirely, or almost entirely, of angular unrolled débris.

When two subordinate valleys join to form a main trunk the two glaciers which fill them also unite into one stream. The result of this upon the lateral moraines is, that while the two outside ones of the two smaller valleys continue to march on and to form the flanking moraines of the greater valley, the two inside moraines join more or less into a single stream of débris which occupies the middle of the larger glacier, and which is known as a medial moraine. It thus happens that a large glacier, in addition to its lateral moraines, often also has one or more medial moraines, and several medial moraines sometimes coalesce. Thus Dr. Wm. Wright, speaking of the great Muir glacier, says: "There are seven medial moraines east of the north and south line, four of which come in to the main stream from the mountains to the south-east. Near the river of the glacial amphitheatre they are long distances, in some cases miles, apart; but as they approach the mouth of the amphitheatre they are crowded closer and closer

together near its eastern edge, until in the throat itself they are indistinguishably mingled. . . . It is the combination of these moraines, after they have been crowded together near the mouth, which forms the deposit now going on at the north-east angle of the inlet just in front of the ice" (*The Ice Age in North America*, p. 44).

These medial and lateral moraines, together with a certain contingent of materials that have rolled down from the flanking rocks and exposed beds, and have travelled farther than the glacier's margin, travel down the glacier's back regularly and continuously so long as the ice surface is continuous and unbroken.

If the slope of the bed of the glacier be neither continuous nor even, but be in some places at a sharp angle and in others at an obtuse one, or be marked by hummocks, the strain upon the ice at certain points will cause it to crack and gape and to form crevasses. These crevasses will remain open so long as the strain exists, but when the glacier gets on to even ground they will necessarily close up again. We cannot understand how crevasses can be formed, or remain open long, where ice is moving over a level or nearly level surface.

The result of the formation of these crevasses, if the ice be not very thick, is that some of the stones which have fallen on the glacier's back will tumble down the fissures and reach the glacier's bed. The rest which escape this and similar catastrophes will continue their journey, as before, to the glacier's foot. This process has been often observed, and is, of course, an elementary experience in the Alps.

The débris falling on the glacier's back consists almost entirely of angular and unweathered stones, but occasionally when the slopes of the hills are partially occupied by old beds of gravel these unweathered stones have a slight mixture of rolled pebbles which have also rolled down on them. The angular stones which are recruited from the decaying crags on either side of the glacier, when they fall to the glacier bed down the crevasses, get rubbed and polished by being pressed against it, and if they happen to fall into the bed of a subglacial stream they are rolled in it as gravel is by other streams.

The whole of the stones, those which have escaped falling down crevasses and those which have so fallen, continue their journey to the foot of the glacier, where they are mixed together, pushed forward, and form masses of heterogeneous rubbish, in which angular and partially rubbed stones are mixed with unassorted sand and clay.

This heterogeneous mixture formed of angular and rolled stones, of gravel and mud, accumulates at the bottom of the glacier in a number of irregular mounds and heaps which are known collectively as a terminal moraine.

All true glaciers or ice rivers flowing down enclosed valleys with overhanging projecting rocks on either hand are, so far as we know, sentinelled by moraines such as I have described, namely, by a terminal moraine and sometimes by two lateral and a medial moraine made up of mixed materials heterogeneously mixed, and for the most part of angular débris. The size of these moraines will depend very largely on the amount of friable material supplied to the glacier.

Terminal moraines are, as I have said, deposited by glaciers at their farthest extent. The ice snout pushes the results of its own denuding work before it and piles it up in mounds and hillocks, the size of which depends, of course, upon the length of the glacier and the amount of materials with which it has been supplied.

If a glacier is gradually and continuously shrinking it will not have time to accumulate rubbish, and will deposit no such mounds anywhere except at its extremity, but will merely strew the valley through which it is retiring with the débris which happens to be on its back and under its sole at the moment. This may include some very large angular blocks and some rolled stones. We can see an admirable example of such conditions on the upper Rhône glacier at this moment. If its retirement is intermittent however, and marked by occasional periods of stagnation, its route will be marked by a number of concentric rows of mounds, a number in fact of successive terminal moraines.

I know of no other deposits made or left by modern glaciers, however big or however small, than those here mentioned, nor can I realise the physical possibility of deposits other than these in the case of any conceivable glaciers.

Now one of the great difficulties of the ice men has been that, in this respect at all events, the instrument to which they appeal, and which they have built up out of their imagination, must have been entirely different to all possible glaciers. In the first place, their hypothesis necessitates the covering up of all available rock surfaces under thousands of feet of ice and snow, where there could not be any available exposed rocks from which to recruit their lateral or medial moraines.

I wish to emphasise this very strongly. When no rocks project above living glaciers, the glaciers carry no stones on their backs. This is beautifully proved by the case of Greenland. In those parts of Greenland where there are no *nunatakker* the Danish observers have noticed that there are no moraines, and it would seem to follow that when, as is postulated by the extreme glacialists, the whole of a country was buried deep in ice, and there were no projecting rocks above its surface, there could be no boulders on its back.

Secondly, suppose the ice-sheets could receive a supply of stones from some transcendental crags overtopping their miles of ice, these stones must, if the ice-sheets acted at all like glaciers, have all travelled on the ice's back and have remained unweathered, unpolished and unscratched, for it does not seem possible to understand how any crevasses could exist in ice-sheets. A great hummock, or hump in a glacier's bed may cause a sufficient strain to crack and open out a crevasse in ice two or three hundred yards thick; but what possible hump or hummock is to cause sufficient tension in a mass of ice even a mile thick so as to make it crack and gape is beyond my prosaic mind to conceive. This stupendous mechanical difficulty is not met at all by the glacialists, but is quite ignored by them.

The fact is, that even in the Alps, where the ice is thick, a great many of the crevasses do not reach the bottom of the glaciers at all, and the stones which fall into them are arrested midway, forming the so-called englacial boulders about which so much astonishing matter has been written, and to which I shall revert presently.

I cannot conceive (as a mere piece of mechanics) how it is possible to understand how crevasses could be formed at

all in those stupendous ice-sheets such as are continually postulated, and how, therefore, if ice-sheets acted like modern glaciers do, any stones could under any circumstances either reach the ice-back or the ice-bed. The ice-sheets would be as barren of stones as the glaciers of Central Greenland are of moraines.

These two difficulties would be overwhelming to most students of an inductive science. They have not been found insurmountable by the glacialists who have a contempt for induction. Inasmuch as glaciers and their ways afford them no support, they turn their backs on them and appeal to purely imaginative conditions.

Thus, to quote one of them, Prof. Dana: "The glacial garment of North America must have been of radiant whiteness, since it enveloped and overlay all sources of spot or stain. From New England to the Mississippi not a peak protruded above its unsullied surface save those of the White Mountains, and they were doubtless capped with snow. The dirty work in progress (and the results show its colossal amount) was carried on out of sight in the basement storey of its lofty structure. Even the business of boulder transport proceeded most likely, in large measure, sub-glacially" (*Manual*, 6th ed., p. 536).

It thus came about that Dana and all his merry crew of supporters willingly and contentedly threw over lateral moraines and medial moraines and surface débris, recruited from crags projecting above the ice's back, from their hypothesis entirely, and elaborated an entirely transcendental and extraordinary theory, namely, that ice-sheets made their own moraines and their own drift entirely out of their subjacent beds. Inasmuch as there are no actual ice-sheets, such as they appeal to, existing anywhere at present, this appeal to cloudland was so far effective that it could not be answered by any inductive process any more than any positive statement about the conditions of the other side of the moon can. The only answer available was either to appeal to analogy or to the laws of physics, both of which are cordially despised by the glacialists. Let us see if this is not literally the case. The problem they had to face was first to explain how the materials of the drift were obtained, and, secondly, how they were distributed.

In regard to the former matter they first postulated, as I have said, that an ice-sheet could break up its own bed by its own pressure, and thus secure a supply of materials out of which to manufacture drift.

Every argument seems to concur against such a view. I have enlarged on the question earlier (vol. i., pp. 424, 425, and vol. ii., 11, 283), and have little to add to what I said. Ice is much softer and more easily crushed than the great majority of rocks, and would itself be crushed and reduced to slush by its own pressure long before the rock upon which it stands could be itself broken. The pressure of the ice again on an ice-sheet would be uniform and not differential. It is not possible to invoke great changes of temperature at the base of an ice-sheet which might assist in breaking the beds by alternate expansion and contraction, for the base of such ice-sheets must have had a temperature which was always very low and always nearly uniform. We must also remember the kind of materials upon which the supposed crushing was effected. These are not lumps of soft rock showing crushed outlines, but clean broken and shattered masses with their surfaces still raw and unhealed, consisting of the hardest crystalline rocks such as granites, syenites, porphyries, etc., as well as limestones, sandstones and chalk, and we are asked to believe that the same ice-sheets which thus shattered such intractable materials *in situ* after passing on a few yards travelled over beds of laminated and stratified sand and loam with such a gentle touch as not to disturb the laminations.

I do not deny or dispute that in certain places (but these are rare, and I have seen none myself) the beds of glaciers are locally broken up. It would be strange indeed if, in a country like Switzerland, where there has been so much dislocation on a large scale everywhere, and so much breakage of the strata, an occasional instance of such breakage did not occur in the bed rock over which glaciers move; but because a glacier is moving over a dislocated rock bed it no more proves that the ice broke up the bed than the fact of a cart rolling over a macadamised road can be said to have broken it up. The word impossible is not a favourite one of mine, but I am bound to say that, if it is to be applied to any physical operation, I know of none where it seems so applicable as to the

process appealed to by the ultra-glacialists for the manufacture of drift by an ice-sheet smashing its own bed.

Having smashed its bed, and thus got a supply of materials, the ice-sheet is next supposed to have proceeded to dig the stones up out of their sockets, and to have thus obtained what the glaciers of our day get from the crags which overlook them. I have criticised this notion in more than one place already (vol. ii., pp. 16, 283), and have only one argument to add to what I have said. It is well to remember what the weight of ice is. "Assuming the specific gravity of the ice to have been 875 compared with water at 1,000," says Jamieson, "then the weight of a mass of ice 1,000 feet thick would be 378 lb. to the square inch, or equal to fully 25 atmospheres, and would amount to 678,675,690 tons on every square mile. If the ice were 3,000 feet thick it would, at this rate, amount to 2,000 million tons on the square mile" (*Geol. Mag.*, 1865, p. 403). It is this gigantic weight against which the stones would have to push and against which they would have to be lifted if they were to be pulled out of their sockets. The process seems as likely as that St. Paul's Cathedral should proceed to dig up its own foundations. It is one of the hypotheses of despair.

Let us proceed, however. Having thus dug up the stones the ice-sheet is then supposed to have distributed them together with the matrix in which they often lie, and it is to this distribution we must now turn.

Let us first consider some of the cases of the exceptional portage of individual stones, and put the well-known case of the raising up of stones to great heights above their original bed rock, in which process they are supposed to have travelled up hill and sometimes very rapidly from their original place of origin and bed rock (see *Glacial Nightmare*, pp. 681-682, and also further on in this work), while in other places the drift has been hypothetically carried over hill and dale irrespective of the drainage of the country. Those who have faced the conditions of the problem most frankly, have seen that no appeals to the ordinary dynamical forces at the disposal of ice when acting under the influence of gravity would suffice to explain these effects.

I have discussed the question of the capacity of ice to

raise stones in this fashion in earlier pages (vol. i., p. 410, etc.), and have little to add. The fact that so many of the raised blocks are angular and unweathered is a great difficulty for the ice men, for it seems to point inevitably to their having been carried, if carried by ice at all, on its back, or otherwise they would be worn and rounded. If this was so, then how comes it that these stones should in certain cases, as in Sweden, for instance, be derived from the lowlands of Dalecarlia and have been lifted on to the Dovrefelds. No ice that could under any conditions have lifted them up on to these mountains could have avoided burying the Dalecarlian country deep down, and such stones could therefore never have reached the ice back.

Suppose we grant, however, that the ice took them not on its back but underneath its foot. How did it proceed?

I have shown that this could only have been done to a very slight extent, if at all, by the ice being pushed from behind by actual direct pressure, since the modulus of cohesion of ice is such that it will crush and refuse to convey such pressure for more than a very limited distance even when not opposed by gravity (*Glacial Nightmare*, pp. 595-598). Even when helped by gravity the stupendous friction and resistance of its bed is a most effectual drag on a glacier's movement. Thus Moseley calculated that the aggregate work of the resistances which oppose themselves to the descent of a glacier of the same uniform rectangular section and slope as the *mer de glace* at Les Ponts, and moving with the same uniform velocity, is about thirty-four times the work of the weight of the glacier in the same time; consequently, it is physically impossible that the mere weight alone of the glacier can be the cause of its descent (*Phil. Mag.*, xl., p. 154).

The only effective motion of a great ice-mass, so far as we know, is, as I have repeatedly urged, the viscous movement of ice as described by Forbes. This movement, as we have seen, necessitates that the upper surface of the ice-mass should have a certain slope, either caused by its bed being a sloping one or by its being piled up in the form of a mound. In order to ascertain the amount of movement of such an ice-mass on its various layers, we have to turn to the mechanics of a viscous body in a state of unstable equilibrium. The

motion in question is merely that of the different layers of this body rolling over each other under the influence of gravity, as a mass of treacle does on a table; with this proviso, that although ice is a viscous substance its viscosity is so very slight that its internal friction when moving is correspondingly enormous.

We know from many experiments how slow is the movement of a glacier on a moderately inclined bed, and nothing is better ascertained than that the motion of glaciers gradually slackens as we get on to less inclined ground and that it presently ceases altogether. Even on inclined ground the greater portion of the motion is not that of the glacier as a whole, but only of its upper layers, which decreases as we go downwards, and, so far as we know, the nether layers of a glacier some distance from its sloping bed are absolutely quiescent. It seems to me that in such ice-sheets as are postulated by the ice men, whose movements are supposed to be quite independent of the slopes of their beds, we must have had on a great scale what we have in a small one in glaciers after they have reached level ground, and after they have lost the momentum given them by their sloping beds. It is for the glacial champions to show how ice-sheets under such conditions can be made to move their nether layers, carrying with them great stones, and to do this for long distances, and then to proceed to climb mountains in the very teeth of gravity. We must always remember that the only force which could be possessed by ice for moving stones lying beneath it for hundreds and thousands of yards up hill would be the residual movement left in the nether layers of such a great ice mound as the ice men postulate. This mound is supposed to have been lying upon a flat or broken surface in which the surface movement (itself very slow) had gradually exhausted its potential energy in overcoming increasing resistance and friction.

No one denies that ice, especially when moving quickly down a steep bed, can overcome certain obstacles like hummocks and humps and override them. This is perfectly consistent with the laws of mechanics, and has been frequently observed. Thus Martins quotes as examples of such movement the glaciers of the Alalein, of the Tschingol and the Grunberg. He also points to the succession of small hillocks

to be seen in descending the valley of the Aar, *i.e.*, (1) those known as the two Baerenbühls, (2) the *roches moutonnées* crossed in going from the Aar glacier to the hospice of the Grimsel, and which form the *contrefort* of the Naegelisgraetli, (3) the Spitalnollen, and (4) the Kirchet, which are marked all over with striæ such as the Aar glacier is still making (Discussion on Durocher's paper, *Etude sur les phen. err.*, etc., pp. 99, 100). What I claim to question is the climbing of high mountains by masses of ice lying on flat or broken ground, and the consequent portage of great stones.

I have tried to further analyse the conditions involved in this view of the older champions of the glacial nightmare on previous pages (vol. i., p. 410, etc.), and to show how utterly they fail to meet the conditions of the problem. The view has in fact been discarded by the younger glacialists, who have accordingly made a still more fantastic appeal to forms and properties of ice quite unknown to most of us. They have invoked the possibility of ice currents in the ice mass itself moving up and down hill and carrying included stones, currents moving athwart each other, some upwards and some downwards, currents not dominated apparently by gravity, but moving quite arbitrarily and urged by some unknown impulse under whose influence great boulders were *persistently* moved up hill, while the great mass of the ice was moving down hill. The process seems to me as likely as that stones should move up hill in the bed or grasp of a mountain torrent.

The notion apparently first suggested itself to Prof. Dana. He discriminated between the movement of the upper and lower portions of his ice-blanket. The inferior parts of the great ice-mass which occupied the valleys and the like he viewed as local glaciers, while he seems to suppose that the superior parts of the same mass were largely distinct from their lower parts. According to him, there was a general ice-sheet which had its peculiar motion and direction, and there were beneath it, and to a great extent separate from it, many local glaciers each having its characteristic features, and moving for the most part independent of the overlying ice-stream; thus there was above and below, at one and the same time, not merely

a difference in position, but also in motion, and the lower part of the mass had a distinct and an essentially different direction (see *Proc. Bos. Nat. Hist. Soc.*, xv., p. 94).

Goodchild quotes Ramsay as having argued that it was not unlikely that the higher parts of a thick sheet of flowing ice might be slowly moving in a direction different from the course taken by the same stream of ice at lower levels, and that two currents might move even in diametrically opposite directions over the same point. "In the vestiges of former ice action in the north of England," he continues, "it seems abundantly evident that such must have been the case." This he bases on the direction of the striæ, and argues that the cases can be traced from areas covering inches to those covering miles (*Ice Work in Edenside*, p. 125; see also *Glacial Nightmare*, p. 679, etc., and vol. i., pp. 413, 414).

Upham, in his paper on englacial drift, speaks of "the slowly rising currents which I believe to have existed in many portions of the base of the ice-sheet," and he considers the conditions most favourable for this upward portage to be the more rapid onflow of the ice-sheet in its upper and central parts, and even in the portion near the ground but not in contact with it, than upon the bed of the ice-sheet where its movement was much retarded by friction (*American Geologist*, 1893, p. 38).

Again, Dr. G. W. Wright, who holds the same view, argues it after this fashion. He says that "owing to the differential motion of the various superimposed layers in a moving mass of ice, the boulders enclosed in it are subjected to a differential strain in which their upper portions are urged along faster than their lower ones". This is of course undisputed. He then goes on to say, however, "that *this must produce a movement slightly upwards*, the result being that the boulders may in this way be gradually lifted up by the ice itself from lower to higher levels". He explains this by the fact that the boulder by intervening a barrier to the ice-flow causes the ice immediately in front of it to stagnate somewhat while the ice above moves on, and consequently the direction of least resistance may be slightly upwards.

I confess, as I have said before, that I cannot follow these arguments. Take the great *massif* of the Alps with its many

valleys choked with ice, each one moving down with inexorable force along its natural bed, and according to the impulse of gravity. Upon this congeries of coalescing glaciers let us plant a mass of ice. How by any known process are we to understand the currents of the ice-flow in the upper layers of the mass following other paths than those dominated for them by the sloping surfaces below which already control the motion of these nether layers?

Again, a mass of ice moving like a viscous body must necessarily have a faster motion in its upper layers than in its lower ones—that is to say, every particle in the upper layers of a mass of moving ice is creeping *downhill* faster than every particle below it. How this differential motion can cause a stone embedded in the ice to move upwards from where the downward movement of the matrix is slowest to where it is fastest, seems to me to pass the ingenuity of any mundane mechanics to explain.

Again, in moving upwards at all, the stone would have to overcome the tremendous vertical pressure of the ice-mass above it, which, on the other hand, would assist any movement it may make downwards. Again, the ice is moving on in the layers below the stone as well as above it; why should the stone therefore not move diagonally downwards as easily or rather much more easily than upwards? Again, we know experimentally that the ice flows freely round all obstacles, and if there is any stagnation in front of a boulder, it must be limited to a very small layer indeed. Apart from this, if there be a tendency thus to move upwards through the ice, how are the postulated ground moraines which are said to have accumulated in places to a depth of hundreds of feet ever accumulated at all, or how does a glacier bed ever become eroded? Stones whose tendency is to move upwards cannot act as scrapers and chisels upon the glacier's bed.

It is quite true that stones sometimes occur embedded in the mass of a glacier, and afterwards appear at its surface, and thus superficially seem as if they had travelled upwards; but this is due to a very different process. If a stone falls down a crevasse, and is arrested before it reaches the floor of the glacier, it will, when the crevasse closes up on level ground, be embedded in the ice. If, again, a stone or a group

of stones lie on the back of a glacier, and a succession of snowfalls come upon it, which snow is converted into ice subsequently, it or they will become embedded in solid ice, and in this way so-called englacial drift may be formed. When by ablation or by the direct rays of the sun a certain quantity of the surface of the glacier is subsequently removed, these embedded stones will appear, like the embedded remains of the famous Austrian travellers lost three centuries before they appeared on the surface of the glacier of Thiedul in 1885; but this is not by any process of travelling upwards through the ice from its bed to its surface, for which no mechanical explanation seems in any way available, but by the uncovering of what was always in mid-ice. What is true of travelling upwards is equally true of travelling downwards, or perhaps more true, for it involves the stones travelling from less to more dense strata of ice, and from layers under slight pressure, and therefore friction, to layers under great pressure and great friction. The stones will, in fact, move with the layers in which they are embedded, although more slowly.

The fact of finding boulders half buried in the ice has misled those observers who claim to have seen stones travelling upwards. Of course they have not actually seen the stones move, they have merely seen them embedded in the ice and appearing at the surface, and they have assigned a quite transcendental cause for it. The question is one of mechanics, to be dealt with by mechanical arguments, and it seems to me to be the height of rashness for geologists who are quite guiltless of any training or knowledge as physicists to appeal to transcendental causes, whose potency they have not tested, and which are treated as contrary to the laws of physics by those specially familiar with the latter.

They habitually argue in a circle. Finding a big stone on a mountain many hundreds of feet above its bed rock, *and having made up their minds*, a priori, *like the schoolmen in the dark ages*, that their *deus ex machina*, ice and ice alone, did it all, they have to attribute to ice qualities which it not only does not possess, but which are contrary to the very elementary laws of matter.

The process of ablation above described naturally tends to

accumulate whatever débris has been entrapped in the mass of the ice in its lower layers, and it thus accounts for the fact that in some places these lower layers are gorged with stones and débris, whose ultimate *provenance* is not from the ice bed but from the ice back. What I am protesting against is the possibility of stones being able to move up and down in glaciers and ice-sheets quite in the teeth of gravity. Whatever explanation is to be given of the raising up of boulders from valleys to mountain tops, this particular one of their having done so through some occult faculty of ice seems inevitably excluded.

These *a priori* arguments are amply substantiated by what takes place in the nearest analogue we can compare these ice-sheets with. Thus Garwood and Gregory write in reference to Spitzbergen with regard to the motion of ice: "We found nothing inconsistent with the view that the flow is due to the action of gravity alone. The one apparent case of uphill advance with which we met may be as easily explained by local subsidence of the upper part of the glacier, owing to diminished snowfall, as by assigning climbing powers to the ice."

Let us now move on again to another feature in the distribution of the individual stones in the drift, which seems to have very effective lessons for us, namely, the divergence of the directions along which they have drifted.

It must be mentioned that we have not to do in this matter with local glaciers which come down different valleys bearing local varieties of rock, and whose course might therefore be radiate and divergent, but with ice-sheets which are supposed to have marched across continents regardless of hill and dale. How was it possible for such ice-sheets to carry stones in directions sometimes at right angles to each other, and sometimes in opposite directions, and to collect them in the same place from all points of the compass, as they are found collected in North Germany, in East Anglia, and in many places in America. The problem quite passes my comprehension. The ice men have invented theories to meet the case, as they invented one to meet the case of the supposed portage of the boulders uphill.

On this subject Mr. Jukes-Browne has spoken with great

effect. Speaking of the Leicestershire drift, he says: "It is obvious that the chalky fragments must have been brought from the north-east, the carboniferous rocks can only have come from the north or north-west, and the marlstone blocks travelled in all probability from west or south-west of the places where they are now found. . . . When we consider the remarkable distribution of the stones and boulders in the clay of this area, the greater proportion of chalk detritus on the eastern slopes, and of jurassic detritus on the western slopes, the fact that enormous masses of marlstone occur many miles to the eastward of the only place whence they can have been derived, the position of the large boulder of Combrash, near Ingoldsby, and the occurrence of lower lias limestone at Croxton, 300 feet above its level, the steep slope of the oolitic escarpment up which the ice must have passed, the difficulties in the way of applying the prevalent land-ice hypothesis becomes considerable" (*Mem. South-west Leicestershire*, Sheet 70, pp. 82, 83). While some have explained these facts by means of ice-sheets, others have assigned them to icebergs and shore-ice. How such agencies could collect oolite blocks from Leicestershire and pieces of *hard* chalk from Norfolk, and mix them with the low-lying Oxford or Kimeridge clays of the Fenland or of the valley of the Ancholme, and then spread them out as we see them spread out from Suffolk in the east into the Central Midlands, passes my belief.

It seems plain that whatever force or engine mingled the chalky clays as we find them, it must have been one that could take up fragments of rock from the north and east and west, and move them in directions opposite and contrary to each other. It must have been able to move not in direct lines and in one direction but in various directions, and thus bring together and mix together the débris from the four points of the compass in one common medley, and having done so could redistribute it as we find it distributed, namely, sporadically, from the low ground of the Fen country where the matrix of the clay occurs. It must have been able to move the oolitic and lias fragments of Rutland, Northamptonshire and Leicestershire to the heights near Southwold in Suffolk, the red chalk, carstone and limestone of Lincolnshire far to

the south of Cambridge, and the chalky fragments from the chalk expanses far to the east into middle England, and then redistribute them over the wide space from Essex to Warwick.

In my *Glacial Nightmare* (p. 753) I have quoted similar cases of the mixture of boulders coming from different directions, from Central England and from Lincolnshire; others again from North Wales (p. 756), from America (p. 764, etc., and especially pp. 769 and 770), and from Germany (pp. 730-732).

Two explanations have suggested themselves to the ice men, one of which seems to me quite arbitrary, and is like the theory by which they explain divergent striæ on the rocks and boulders. They claim that these boulders found together and mixed up together in the same beds are the result of the portage of entirely different ice-sheets, dating from different times and coming from different quarters. For this there is no evidence of any kind known to me, save the necessity of explaining a desperate difficulty by a desperate argument. What could induce ice-sheets to culminate first in one low-lying region then in another, making them foci of distribution, and to move at one time in one direction and at another in a different one altogether, apart from the lines of least resistance and the contour of the surface? Surely again we ought in such cases to find a succession of beds marked by different kinds of stones and not a hotch-potch.

This view is maintained, however, by only a few. A more numerous body of glacialists have had recourse to another and even wilder explanation, namely, that of divergent and convergent currents in the ice-sheets, which are supposed to have occasionally flowed at right angles and even in direct opposition to each other, and sometimes in combination with the movement of the stones up and down the ice-sheets.

Goodchild's is not a bad example of this kind of reasoning. Thus he says: "On the outskirts of the Lake District there are many instances of boulders of certain rocks that have first travelled outwards under the influence of the local currents, and then, as they worked their way upwards in the ice and came under the influence of the upper currents that were setting inland from the Solway, they were swept back

again on the ice towards the heart of the mountains. . . . In the Eden valley there are abundant instances of this cross-transportal of drift materials" (*Ice Work on Edenside*, p. 501).

Goodchild especially mentions how in consequence of his cross currents in the ice, stones from Galloway (Criffel granite, etc.), Shap granite and other detritus from the north side of the Lake District, stony matter from the Cross Fell escarpment and materials transported in various directions from the higher parts of Edenside were mixed with each other and with local débris. He also calls attention to certain areas in the north of England that were never glaciated by ice, and agrees with Ward that there is no clear proof that any polar ice swept clean over the Lake District, nor over the other mountain masses in the north. He attributes the inland and uphill movements of the ice to the stoppage of the natural outlets for the ice deploying upon the Solway, which, however, was not the Greenland ice, but was propagated in a south-east direction by accretions of local ice. In the Lake District the bottom layers of ice radiated out from the high lands and maintained their initial direction of movement in some instances miles away from their starting-point, as is shown by the glacial striæ. Such striæ occur as far as Blencow, near Penrith, and at Skelton still farther off, and clearly point, he says, to glaciation outward from the Lake District. He urges, however, that while this was the case with the bottom layers of ice, other layers had differential movements, which brought to one place materials from all points of the compass, and occasionally transported boulders at a high level in opposite directions to that of their initial course.

All this seems to me mere dreaming, and I hold it to be perfectly monstrous that geologists of weight, especially official geologists, should spin cobwebs of this kind without the slightest attempt to correlate their results with any causes which the physicist would tolerate as possible.

It will be remembered that the champions of this theory of internal currents in the ice are also those who claim for it the feature of moving at times in such straight and rigid lines that it has been able to striate scores of miles of country with a broken contour with perfectly straight striæ.

So much for the theories which have been invented to explain the movement of boulders from lower to higher levels, and the converging of rocks from many districts to certain particular spots.

Let us now turn to another puzzle, namely, the explanation of the existence and transport of great tabular masses of rock, especially chalk, sometimes several hundred yards long, embedded in the drift beds and padded all round by sand or clay, of which the typical examples occur in Norfolk, where they may be seen lying in the so-called contorted drift in the Cromer cliffs, of different sizes, from seven or eight yards long to as much as 180 yards, but seldom exceeding four yards in depth. They also occur inland.

Thus Prof. Judd (see *Geol. Surv. Maps*, Sheet 64, Rutland, etc.) says: "The transported masses of local rocks are sometimes of enormous size, especially in the northern portion of this area and in that to the south. The attention of geologists was first directed to these great transported masses by Prof. Morris, who found that at the south end of the Stoke tunnel on the Great Northern Railway an enormous mass of Lincolnshire oolite limestone lay on undoubted boulder clay. During the mapping, by Messrs. Holloway, Skertchley and myself, of the districts which I have indicated, we have found a number of such transported masses, some of them far exceeding in size that described by Prof. Morris, and composed both of the inferior oolite and the marlstone rock bed. . . . The largest of these transported masses, that capping Beacon Hill (on Sheet 70), is more than 200 yards across, and is composed of the marlstone rock bed. It is noteworthy that these masses always belong to the rocks which form the highest ground" (*Geol. of Rutland*, p. 246). The mass of oolite above referred to as having been described by Prof. Morris is, he says, 430 feet long and at its deepest part thirty feet thick, and is underlaid by seven feet of drift (*Quart. Journ. Geol. Soc.*, lx., pp. 318-320).

These great masses of shifted rock occur, of course, elsewhere. Notable examples are those at Roslyn Hole, near Ely, consisting of a mass of chalk, gault and upper greensand, over 480 yards long and forty-four wide, the great Merton boulder in West Norfolk, and more remarkable than all, the

great mass of chalk, twenty miles square, recently found by Mr. Cameron on the borders of Huntingdonshire and Bedfordshire.

Similar phenomena have occurred in America. Thus Winchell describes how "in the north part of Linawee and Hillsdale counties, and the south and east parts of Jackson, the south and west parts of Washtenard county in Lower Michigan, there are found numerous tabular detached masses of limestone, sometimes cropping out on the hillside like a ledge in a plane, and sometimes embedded two or three feet in sand and gravel at the summit. They are generally for the most part slightly tilted in one direction or another. They are sometimes six, eight or twelve feet square, and sometimes many hundreds of bushels of lime have been burnt from them." Numerous special instances are described in detail. "These tabular masses have not been rolled. By some agency they have been lifted gently from their original sites and carefully deposited where we find them. . . . The abundant gravel remains found in them show they belong to the corniferous limestone, as exposed along the west shore of Lake Erie and at numerous points throughout the county of Mourse. . . . These masses have been transported, for they are found resting over the Hamilton group, the Marshall group and the carboniferous limestone, and I am pretty well convinced, even in some cases, as far north as the coal measure." Winchell urges that these masses have not been transported from the northern outcrop of the rocks in North Michigan: (1) Because the transporting force has moved no stones so big; (2) because they would have been broken up, worn and rounded, if so transported, if we may judge from what happened to the crystalline boulders; (3) if they had been so the harder and more massive Niagara limestone of the same regions would also have been transported, so with the Trenton limestone. Small fragments of the corniferous limestone hardly occur at all, while the remains of the other rocks just mentioned are exceedingly scarce. He derives them from the south where the corniferous limestone crops out in North Indiana and Ohio and the south-east corner of Michigan, and says that they have been transported northwards. The formation dips under the

peninsula of Michigan, and throughout the area occupied by the masses in question it lies from 100 to 1,200 feet beneath the surface. Boulders of the Hamilton group found with the masses have apparently travelled the same way, so with nodules of kidney iron ore and pieces of Marshall sandstone.

Winchell points out that in Alabama "the red loam," which is the rotten limestone of the cretaceous series weathered *in situ*, has been moved northward over the clayey and sandy region appertaining to the lower cretaceous (*Amer. Journ. of Science*, xl., pp. 331-338).

In a further paper the same writer refers to other instances of the same kind in masses of carboniferous limestone from ten to sixty feet in length, and often of unknown thickness, floating in the sands of Oceana County, apparently 100 or 200 feet above the bed rock; some, he thinks, may be *in situ*, others are demonstrably severed and displaced portions of it. The sands in question are semi-stratified, and the masses have travelled northwards (*ibid.*, xl., November, 1865).

These gigantic boulders have been a great puzzle to the ice men, and have led to the most extraordinary mental gyrations in trying to explain them. The explanation of their detachment from the base rock is as difficult to them as that of their transport. The land-ice men have invoked their notions about ice smashing its bed, which we have already criticised and which seems particularly inept in the case of these long, narrow, ribbon-like strips of chalk, and sometimes of chalk attached to gault, etc., and detached intact with the rows of flint in place. How the surgical or mechanical operation could be performed passes all belief, especially when we remember that this same ice-sheet in this same part of England is supposed to have travelled over finely laminated beds without disturbing the laminations. The agent whose gentle tread is thus attested, is supposed, in the words of the extreme champions of ice, to have "forced up the chalk beds into a long fold, which, under continued pressure, was gradually bent over in the direction of movement, until the top of the inverted fold was sheared off and the detached mass driven up an incline and forced into the overlying ground moraine"!!! Is this science, or what is it?

It is just as difficult to understand the transportation of these masses.

Speaking of these enormous chalk boulders, Mr. Mellard Reade says: "How horizontal masses of such longitudinal dimensions could be conveyed and embedded in mud and sand with so little fracture is one of the suggestive problems presented for our solution. . . . It is plain to see that, as a rule, the junction of the external surface of the chalk with the drift in which it is embedded is marked by laminated beds of the drift a few inches in thickness, enwrapping the mass or boulder, as shown in the numerous examples figured. The most surprising thing about the boulders is perhaps the length and extreme tenuity some of these masses attain, together with the remarkable way in which the continuity of the chalk is preserved without disruption. . . . A mass between Cromer and Sheringham is about 120 feet long; I judged it to be about twelve feet in its thickest part, thinning out gradually northward at its lower end, where I measured it, to only nine inches. This part I could reach with my hammer, and found it to be solid, compact chalk. The flints follow the form of the mass, showing that it has been to some extent bent, but I could discover no fractures. The most remarkable fact I have to record as bearing upon its mode of transport was a bed of shells about two or three inches thick, consisting of broad valves, closely packed and crowded together, and above it was a similar bed. . . . Where these shells occurred the boulder was underlain by contorted beds of sand and above by sands widely stratified in the direction of the upper surface. . . . Lyell in his paper in the *Philosophical Magazine*, 1840, shows also how the lamination and folding of the drift in contact with the masses conforms to their outline."

While it is quite plain that no masses of included rock of these vast dimensions occur of the harder and primary rocks, the sandstones, gneisses, granites, syenites, etc., masses of marlstone, rock bed, of oolite, and of chalk and gault of enormous proportions have been described, as we have seen, by Morris, Judd, Skertchley, Bonney and others, as found embedded in the boulder clay of Lincolnshire, Rutland, etc. These all possess one common characteristic, *viz.*, that they are long, shallow and narrow. In regard to their mode of trans-

portation, Mr. M. Reade says: "The transporting agent, whatever it may have been, could convey them for many miles. Prof. Judd considers that some of the boulders he describes from the marlstone rock bed have travelled not less than thirty miles. Land-ice, he says, is out of the question; the effect of a glacier is to grind its bed into an impalpable mud; and I cannot conceive any means by which an ice-sheet could tear up masses from the bed of the rock it passes over and detach them in this way. An iceberg is only the detached termination of a glacier or a sheet of ice, and the same difficulty applies to this mode of origin, with the additional difficulty of requiring a great depth of water for flotation or transport. . . . That a mass of this shape and form could be pushed up, enveloped in drift, by a Scandinavian or any other ice-sheet, and preserve its continuity undisturbed, appears to me to be all but inconceivable."

Mr. M. Reade then goes on to suggest that the masses were broken off cliffs or escarpments and rolled or fell upon pack-ice or shore-ice. He says: "Pack-ice driven on to the shore and piled up over and around it, and then in winter frozen into a sheet or ice-foot, might assist the accumulation until a raft were found sufficient to float off boulders of the largest dimensions".

In illustration of his argument he quotes Mr. Trimmer, who says: "Sir Edward Parry found for miles along the coast near Melville Island a dark blue stratum of solid ice, embedded in the beach at the depth of ten feet under the surface of the water. While Dr. Rae describes how at Repulse Bay, during the spring of 1847, boulders situated at low water-mark were frozen into flow-ice, and being lifted by the rise and fall of the tide became eventually encased in the ice which he found to measure eight feet thick. In the spring, by the double effect of thaw and evaporation, the upper surface of the ice was removed, so that the stones that were formerly at the bottom of the sea now appeared in the surface of the ice as if dropped there from a cliff" (*Quart. Journ. Geol. Soc.*, 1882, pp. 229-235).

Mr. Osmond Fisher calculates (*Geol. Mag.*, 1868, p. 550) "that flotation cannot take place unless the earthy matter does not exceed one-twentieth in bulk of the whole mass.

If therefore a mass of chalk 600 × 60 × 60 feet is to be rafted off, it would require ice equal to 432,000,000 cubic feet including the rock to effect it, or a combined mass 1,200 feet long by 600 feet broad and 60 feet thick. That such a raft is a possibility it would be difficult to deny. With an ice-raft of this nature resting upon the inclined plane of a shore, and subject also to the lifting power of the tides, it is not difficult to conceive how such a mass once set in motion would launch itself out into the sea."

"In some such way," says Reade, "I conceive the puzzling 'boulders' have been derived and transported, and it is readily realisable that with a large superficial raft of ice the melting power of the sea-water acting on an extensive surface might soon cause them to founder. In many cases the sinking has been very gentle, or the mass would be more fractured than it is. . . . But the vertical displacement is not, in my opinion, sufficient to account for all the contortions of the drift in which we find them embedded; . . . lateral force has had a great deal to do with the folding of the strata. I conceive that the drift of Cromer formed a large submarine bank, and that the sea was sufficiently shallow to allow of the ice-rafts and their burdens often grounding. If this be so, it is not difficult to see that the impact of a mass of ice and rock weighing 1,200,000 tons would be quite sufficient to disturb, bend, fold and contort the stratified beds of yielding mud, clay, sand and gravel. If such ice-masses with their burdens sank into yielding mud vertically, or were driven into it by lateral pressure, the melting of the ice encircling the boulder might account also for some of the contortions of the surrounding beds. I have shown that the drift frequently has an apparent stratification, concentric with the surface of the boulder, and not only encircling, but frequently penetrating it."

In the discussion that followed, Prof. Judd said he agreed with the author that the transported masses must have been conveyed by floating shore-ice, and could not have been carried by any form of land-ice or iceberg. The Rev. S. Hill found it difficult to understand how the rock masses could be separated from cliffs without being overturned (*op. cit.*, pp. 235-237).

I am bound to say I cannot follow the arguments used by my distinguished friends on that occasion. In the first place, it seems incredible that the grounding of an ice-mass on a chalky cliff should detach long, ribbon-like masses of chalk with their layers of flint intact, or, in fact, detach them at all. Nor can I see how by any possible means icebergs, or floating ice of any kind, could either get hold of, or move about, or transplant these enormous cakes of chalk, oolite, etc., many hundreds of feet in length, or underlay them with soft drifted sand and clay. If they were foreign bergs or floating ice, how could they by any means take up or detach these monster boulders? They could not get them on their backs, for the chalk is at the sea-level; how could they attach them to their feet? Floating bergs cannot get frozen to anything. What applies to bergs applies also to shore-ice. Shore-ice will, no doubt, move stones along shore, and sometimes push stones up sloping beaches, but it cannot take up great masses of chalk and deposit them in beds of loamy sand with their laminations intact. Besides, shore-ice implies a shore, and how was it possible to have a shore at Cromer or in Mid-Lincolnshire when the hypothesis must be applied to the great stones in Rutland and other inland counties if they have to be similarly explained? Nor are there any traces of shingles or old beaches where these chalky cakes occur. Mr. O. Fisher, who approaches the problem after calculating the conditions, requires ice-floes sixty feet thick, which would bring the conditions of the Palæocrystic Sea into the latitudes of Lincolnshire and Norfolk. How, again, could the North Sea, if so frozen, have any tides or any shore action such as is suggested?

To return, however, to the land-ice men and to other of their transcendental modes of boulder transport, and especially to their cross-currents in ice.

How the kind of reasoning they patronise on this subject is tolerated, and how it is possible to call it science, I do not know. To import what takes place sometimes among slightly aggregated forms of matter, such as liquids and gases where cross-currents frequently occur and can be easily explained, into matter in virtually the solid state like ice, and to invoke internal currents flowing in different directions apparently uncontrolled by gravity in it, seems to me quite fantastic.

It is quite true, of course, that when a river of ice is coming down a valley and is recruited by an influent current coming down another valley making a considerable angle with it, the smaller current if sufficiently rapid will cause a back-flow or surging up of the main stream at the point of in-flow, and for a little space, and until the two ice-streams have finally coalesced, there will be divergent currents; but these are perfectly explainable as the result of ordinary mechanical forces, and present no puzzle any more than the splitting in two and local divergence of an ice-stream in the presence of some projecting rock, and the convergence of the two halves of the separated stream after the obstacle has been passed. But this has nothing to do with a theory which sees in the interior of a glacier movements of the particles impelled by some unknown and transcendental force coursing through it in different directions either at the same or at different levels.

Another feature of the drift, which is a puzzle to the ice men, and which has been noticed in many countries, is the fact that the unrolled and unweathered stones are roughly sorted according to their size. There is an occasional exception, but the great proportion of them grow smaller and fewer as we get away from their original *provenance*. This seems quite inconsistent with their having been carried by ice, since it is the peculiarity of a glacier that it carries a stone as big as a house just as easily and as far as it carries a pebble, and in the case of true glaciers, as in the Alps, we find examples of the very biggest stones at the farthest points reached by the ice. This sorting seems consistent only with the action of water and quite inconsistent with that of ice in any form.

Again, how by the action of ice in any form are we to explain the alignments of great stones so frequently found and which have been described from Russia, Germany and America? This regular and symmetrical arrangement of rows of big stones is surely quite incompatible with ice-sheets and their hypothetical method of sub-glacial portage. We can understand lateral moraines or medial moraines in certain cases being formed of such alignments, but not the stones carried along beneath its foot by a stupendous ice-sheet which

ex hypothesi had no such moraines and in which no kind of regular arrangement of the materials seems possible.

Lastly, a word or two about the poised or suspended stones.

Many of these stones are perched on the bed rock with such nicety that they can be made to rock. How under any conditions of ice portage this was possible, I do not know. If the stones were carried on the backs of the ice-sheets and rested where they are when the ice melted, it would be a nice calculation how many million chances to one there would be of their settling down and remaining thus poised in such numbers as they are. If, on the other hand, they were carried under the ice-sheet and thrust along by an exceedingly heavy slow pressure, it seems to me the chances could be equally measured. Whatever their cause, ice seems quite out of the question.

Turning from the individual stones to the drift when massed as a whole, the problem of its formation and distribution on the hypothesis that ice had to do with it, seems daily more difficult to understand. The products of glacier erosion are very easy to find and to dissect in Switzerland, Norway, etc. Their great feature is their heterogeneousness of contents and structure. They consist of a mixture of sand, clay and stones in which there is no sifting of materials, but they are mixed together without any order, and are what the Americans call dirty. Ice has no power of sorting materials into clay, sands, etc. It crushes and pounds and rubs them down, and then lays them down altogether, as we see them laid down in glacial moraines.

Now the most obvious feature of the great mass of the so-called glacial drifts is that they are sorted, that the clays have been laid down separately from the sands, and that in many cases they are more or less rudely stratified. It is only in exceptional cases that we find them laid down in the form of heterogeneous dirt or muck, *i.e.*, as true moraine stuff.

Even when thus thrown down heterogeneously, as we shall see presently, it by no means follows that it was ice which threw down the materials; but what seems perfectly certain is that ice could have had nothing to do with the sorting and sifting of the beds into their several contents, nor with their arrangement in laminæ or as stratified deposits.

The glacialists for the most part see this, and they accordingly appeal to the action of sub-glacial streams as having supplemented the action of the ice-sheets themselves. In regard to this appeal it seems to me as oblivious of the concrete facts as most of the others made by the same people.

When these great ice-sheets existed it seems impossible to understand how there could be anything but very slight sub-glacial streams, if any at all. The conditions postulated involve such a desperately wintry climate that we cannot well understand the melting of the ice in order to form such streams. Whatever melting there would be would come from the effects of solar radiation acting on the surface of the ice in summer, which, under the conditions of continental stretches of snow-white ice, would be only slight, since the greater part of the direct solar heat would be reflected.

While the sun's rays would be thus largely reflected, how long would it take for the fiercest solar radiation alone to cause the ablation of such vast masses of ice? And yet it is argued that they melted so rapidly that they could sometimes disappear altogether without laying down a series of terminal mounds and leave the country covered with a continuous ground-moraine of gentle and more or less uniform outline. Surely the whole notion is preposterous.

Secondly, as there could be no crevasses in these portentously thick masses of ice except under very exceptional circumstances, the ice-streams we are accustomed to in the Alps, in Norway and Greenland, would be quite out of the question, for they are almost entirely fed by the water which falls down crevasses.

Thirdly, even if we could secure such water on the scale needed to produce the effects, it would not sort the soft materials like clay and sand in separate beds overlying each other, and in so many cases stratified. The water in question would do what sub-glacial streams do now, namely, wash away and scour all the soft and light materials, and convey them far away, leaving a bed of clean boulders and gravel such as we can see in most of the higher Alpine valleys which receive glaciers.

No doubt we occasionally find small patches of more or

less slightly stratified materials in modern moraines, but these are mere patches; the great mass of the moraine material is heterogeneous rubbish, pushed before it by the snout of the glacier.

Again, the ice-streams of modern glaciers follow definite lines bounded by definite courses, and flow, not indiscriminately under the whole of the glacier, which they could hardly do, but in definite tunnels which they have made in the ice. It is not possible to understand how such narrow streams could lay down the widespread beds of stratified drift. I cannot understand, again, by what possible process the portentously rapid melting of the ice front, postulated by the American geologists to account for what they call the Champlain period, could be brought about. The outpouring of sub-glacial lavas has been suggested, the outbursts of volcanoes has been similarly referred to, but these are the most arbitrary of hypotheses, literally without any evidence to support them.

Another enormous and in fact insurmountable difficulty in attributing the stratified drift to sub-glacial streams of any kind is its distribution quite independently of the contour and drainage lines of the country. Whatever gymnastics ice has been credited with by its champions cannot be extended to rivers and streams, which, unless contained in watertight tubes, cannot move up and down hill and disregard the contour of the country.

Let us now pass on from the structural features of the drift to its mode of distribution. If the drift was distributed as we find it by ice, it must have been by ice in motion. I have frequently challenged the glacialists to show how the nether layers of their enormous ice-sheets could move at all. Supposing we could secure such a vast ice-mound as they continually appeal to, we may perhaps imagine its surface layers having a slight movement whenever its upper slope passed the angle of equilibrium; but how this motion was to be conveyed down through successive layers of ice (with increasing friction caused by the weight of the ice itself and its difficulty in shearing), so as to retain any appreciable movement in its lowest stratum, passes my comprehension, and this *a priori* view is amply confirmed when we test the problem by actual glaciers which gradually slacken and stop

and become as quiescent as the lower layers of a lake when we get on to level ground.

Suppose, however, we grant that effective movement would not be exhausted in the upper layers of the ice-sheet, but be maintained even by the lower ones—a postulate which would, I think, be scouted by most physicists—how is it to work so as to produce the effects demanded from it by the glacialists?

An ice-mound moving under the influence of its own viscous tendency must move sporadically from its culminating point in all directions, and we cannot quite see how, this being so, we are to secure the movement of stones in parallel lines over great stretches of country and in a straight course. A glacier flowing down a narrow valley no doubt would move stones in parallel lines in this way, but the very nature of an ice-sheet necessitates its movement being radial. How, again, can we understand the thrust being so unequal? Take the so-called Scandinavian ice-sheet which must have culminated in North Sweden. How is it that if it could carry stones thence to the Carpathians and from Finland to Central Russia, that it should only have been able to carry similar stones westward as far as the row of islands bordering Norway and northward to the shore of the Arctic Sea and no further? Similarly in America with the Hudson Bay and the Laurentian ice-sheet. I cannot understand this at all.

Let us again turn to the supposed *modus operandi* of these ice-sheets. The glacialist claims for them that they polished and striated rock surfaces wherever they went, and also produced the striated and smoothed stones and the clays and sands of the drift beds by the action of their own denuding feet. At the same time they claim that these ice-sheets also carried and laid down the drift itself.

I cannot see how when engaged in polishing and moulding the surface of the land, as they are supposed to have done, they could have applied the chisels and planes with which they worked to the bed rock which they are supposed to have worried, when between that bed rock and the bottom of the ice there was a continuous cushion of sand, or mud, or clay. If this presents a difficulty in regard to the polishing and striating of the bed rock, it surely presents an equal

difficulty in regard to the smoothed and scratched boulders, which are enclosed in a matrix of soft materials like raisins in a pudding. It is incredible to me under any theory that the work of denudation could have been done concurrently with either alternative.

I fancy this would be admitted by most of the more reasonable glacialists. The denuding of the rock surfaces and the making of the boulders must have been an antecedent process to their being covered over with the mantle of drift, and it has in fact been urged that it is only when advancing that ice is an active denuding agency, while when retiring it becomes a depositing one. In the former case it is a dynamical agent, while in the latter it has ceased to be so, and is merely leaving behind the relics of its former activity. This sounds very plausible until we come to analyse the process by which an ice-sheet could alone move. This was entirely by the drag of its layers, each one acting on the one below it in consequence of the viscous mass (to which additions were continually being made by additional snowfalls) gravitating down to a state of equilibrium. If such an ice-mass as the glacialists demand was possible, which I altogether doubt, it was only on condition that when it ceased to grow it ceased to flow and became stagnant altogether. How under these conditions could it accumulate anything under it? Where was the material to come from? So long as its nether layers moved and scraped along the bed, we may understand that some débris would be formed by the denuding process; but directly this movement ceased, the making of drift material would cease also, and there would be nothing to accumulate when the ice melted. The condition of things in an ice-sheet must have been quite different to those in a glacier which, whether advancing or retiring, has always a gravitating tendency imparted to it by its lying on a sloping bed; but with an ice-sheet that was gradually melting away the impulse to any movement would be gone, since it is only by growth on its surface layers that the condition of want of equilibrium would be produced. I cannot therefore understand how any accumulation of drift would take place at all under a retiring ice-sheet.

It is only some of the ice men, however, who appeal to the

activities of ice-sheets when retiring. Another set retain the view that the ice-sheets deposited their débris when advancing only, and that in some unaccountable and quite transcendental way they at one time denuded their beds, and presently when consisting of the very same ice, acting on the very same place, they laid down soft materials which they in some way dragged along with them not only in thin layers of sand and clay, but in tremendous beds many hundreds of feet thick.

How is it conceivable that ice could urge and carry along disintegrated materials like these? They must have been either frozen or unfrozen. If they were frozen it seems to me they must have been permanently frozen, for under a mile of ice the vicissitudes of summer and winter would not be felt. If so, how can we account for the sub-glacial streams without which the glacialists' theory cannot work, since so much of the drift is sorted and stratified. If unfrozen, I cannot see how ice could possibly grip and carry along with it slippery clays and disintegrated sands and similar loose materials, not a few inches but in many cases hundreds of feet thick, and carry them with it for hundreds of miles up and down hill into deep troughs and out again—the very same ice, be it remarked, which is supposed to have scarified and polished and worn down the rocks on which these beds lie.

These vast thicknesses of the drift in places seem in themselves to me to be a very effectual argument against the fantastic theory of ground moraines.

The same result seems also to follow from the fact that these drift beds are found in the mountain valleys choking them right up to their heads. Whatever may have been the case elsewhere, the ice must surely in these mountain valleys have followed the lines of least resistance and moved down their natural roadways and cleaned and swept them out, and not left them gorged in this way with such débris. It is in these upper valleys that the greatest amount of denudation ought to have occurred. How comes it then that they are choked in this way if this was the case?

Again, how are we to account for the fact of the drift being similarly found in great beds on the crests of many of the passes in the secondary ranges, as in the Grampians, where it must have met with the most potent opposition from, and

been swept away like a fly on a plate by, the tremendous machine which is supposed to have made the vast deposits of the so-called glacial age?

The very continuity of the drift mantle with its smooth and evên surface over such wide and irregular areas is another insuperable fact against any ice-sheet having had to do with its deposit. No glacier anywhere known to me deposits such a mantle either in advancing or retiring. A glacier in advancing sweeps out its bed and pushes a moraine before it, and whenever we can examine where it has been, we find no traces of such a continuous ground moraine. Upon this Bonney and other experienced Alpine explorers are at one. If a ground moraine is under any conditions possible, it seems to me it can only be with a retreating glacier or a retreating ice-front. And this I have already treated as incredible and impossible to realise unless the retreat of the ice was perfectly continuous. Every halt must have caused a piled-up moraine, not a sheet of drift with smooth, flat surface. I may add that those glacialists in America who are, by way of discarding the theory of ground moraines, in favour of the deposition of the continuous beds as a simple deposit of so-called englacial drift left *in situ* by the melting ice, fail to tell us how in this case so much of the drift should be sorted and so much of it stratified. Englacial drift settling down from a melting ice-sheet must be heterogeneous, which such a large part of the drift is not. These facts and arguments seem conclusive when we go into the field and leave our books. We then find that, except near mountains or in broken country, the drift is laid down in softly outlined continuous mantles and beds implying a widespread depositing agent which did not leave its burden, as glaciers do, in heaps and confused mounds, but in smoothly contoured sheets, extending over wide districts in a continuous mantle independent of the surface contour of the country. I say continuous. In certain remarkable instances, however, the deposition of the drift was not so continuous, but we have large areas forming huge islands in the drift-covered regions entirely free from any drift at all, and free also from any of those manifold signs which are usually accepted as the ear-marks of glacial action. A very famous instance of this is the so-called driftless region of South-Western Wisconsin, another is in the

Government of Orel in European Russia. Dr. Wm. Wright says of the former, "there is an area of several hundred square miles in extent, occupying more or less of the adjoining area in Illinois, Iowa and Minnesota, which remained as an island in the great continental expanse of ice. The ice," he says, "moved past it upon both sides, and then closed together upon the south and moved onwards a distance of about 300 miles to the vicinity of St. Louis" (*Ice Age in America*, p. 120). Mr. Carvell Lewis claims to have found driftless areas even in Northern England. I know of no explanation coming from the glacialists (which professes to meet this extraordinary case) which does not seem quite hapless. Dana suggests, for instance, that the driftless area in America is still one of light precipitation, but the ice-sheet that carried Laurentian and other rocks to St. Louis was surely strong enough and mighty enough to dispense with such aid as local precipitation in Wisconsin. If the district had been a high plateau or congeries of hills, we could have understood it perhaps by invoking the mechanical laws by which moving water or ice would pass round an obstacle as the road of least resistance rather than over it. But even this would not have availed to the ultra-glacialist who claims that his ice-sheets were capable of moving independently of the contour of the country.

Let us now proceed. The drift beds are not everywhere laid down in continuous sheets and mantles. In many cases they are heaped up in mounds and ramparts, which have a very material lesson for us in the issues we are engaged in discussing.

It is only in more recent times that real care has been taken to classify these mounds, and to examine their internal structure as well as their external contour, with the result that large classes of them have been directly assigned to the action of water and not of ice, and the proportion of those which in structure and arrangement of contents resemble real moraines has been greatly reduced.

In two respects, however, this analysis seems to me to be faulty, and to involve a faulty induction.

In the first place, a great deal of the drift which is obviously stratified has been assumed to be re-arranged and re-deposited

drift, and in some way to be different in age and character from that of the unstratified heterogeneous drift. The view that it is so has been forcibly protested against by Prof. Dana, who objects to its being taken for granted that the stratification which it presents is not an original feature of the beds, but has been subsequently imposed upon them in some way, and that where we find traces of such stratification we have evidence that the beds have been in some way re-sorted and re-arranged—*remanié* as the French call it. Against this notion I also entirely protest, nor do I know any evidence for it. On the contrary, I believe most firmly that the stratified and unstratified drift belong to the same period, were due to the same ultimate cause, and, as we have seen in earlier pages, they grade into one another. Inasmuch as stratification and lamination are inconsistent with a deposit by ice, and only consistent with a deposit by water, it follows that when we find large masses of deposit, portions of which are stratified, we cannot treat them as moraines at all but as aqueous beds.

On the other hand, as I have already argued, and propose to argue again presently, the fact of a bed being heterogeneous in structure does not preclude its having been thrown down by water. The way in which water deposits materials depends very largely upon the rate at which it is travelling, and the mass of the water itself. If it is travelling rapidly and in great mass, it does not sort its materials but carries them altogether, and when interrupted it throws them down together in a mixed fashion. Thus while stratified beds are quite inconsistent with ice deposition, unstratified ones are not inconsistent with water deposit; and where we find the two sets of beds intercalated or running into each other as they do in so many cases, and in such a way as to make it obvious that they are contemporaneous, we have no other resource than to attribute their deposit to water acting at one time tumultuously and at another gently. Ice cannot claim such phases.

Let us now turn to the more conspicuous drift deposits to which this latter argument specially applies, and which have, in consequence, been largely treated as aqueous beds; and first let us consider the most extensive and remarkable of

them, which are called äsar by the Scandinavian geologists (whence the term has been largely imported among us), and eskers by the Irish peasants and the Irish geologists. Esker is not only a more euphonious name, but has distinct claims to priority. It is of Celtic origin and very old. Thus Sollas says of it: "The series of esker groups to be described later on is part of what was formerly called by the general name of Esker Riada, which is mentioned in the annals of Clonmacnoise as the southern boundary of the kingdom of Conn of the Hundred Battles" (*Trans. Royal Ir. Acad.*, Series ii., vol. v., p. 787). In America these ridges are also known as Devil's Backs and Hog's Backs. Their explanation has long been a crux and perhaps a reproach to geology, and it remains so still. Thus one of the latest writers on Swedish geology, and one of the ablest, Nathorst, in his *Sveriges Geologi*, published in 1894, after examining the various theories which have been forthcoming to explain them, has to confess that the problem is still unsolved. To use his own words: "Vilja vi dock på samma gång uttryckligen betona, att vi ånnu icke betrakta frågan såsom slutligen afgjord" (*op. cit.*, p. 243)—"we must expressly state that we cannot consider (or look upon) the question as finally settled".

The äsar are such a notable feature in the landscape of Sweden that it is not surprising they should have been observed and their peculiarities described at an early period there. Their main features were, in fact, pointed out by Swedenborg at the beginning of the eighteenth century, and have been enlarged upon by every succeeding explorer. The Swedish geologists divide the äsar into two classes—the äsar properly so called, built up of masses of rolled stones, and the sand-äsar, composed chiefly of sand. While it is easy to find specimens of each of these, it is also very easy to find others where masses of rolled stones and beds of sand or of tough clay or brick-earth pass into each other very much as they do in the Cromer cliffs. A good example is the fine äs upon which Upsala is built, and in which we can study the internal structure admirably, since it has been recently excavated right through. I gave a figure of a section of it in the *Geological Magazine*, 1898, Pl. VII.). There

we can see in the course of a few yards the passage from a mass of rounded boulders into sand. The sand in some places is almost continuous, and in others has banks of clay intercalated in it. The contour of the äsar, as Swedenborg long ago pointed out, differs with the nature of their contents, the stony äsar having steep sides, while the sandy ones have much rounder outlines. The stones which form such a great part of the äsar (except certain specimens occurring in their upper parts) are invariably rounded and water-worn, and would be well described by the phrase applied to some of the East Anglian gravels, *viz.*, "cannon-shot gravel". The äsar are found in all parts of Sweden from Scania to Norland, and in Finland and Northern Russia they form, as is well known, huge banks and ramparts. In some cases they run with great uniformity in shape and breadth for long distances, their direction being wonderfully continuous. So uniform are they that, as Brongniart pointed out, the roads in some places, as from Upsala to Wendel, from Enköping to Nora, from Hubbo to Moklinta, etc., run along their crest. Sometimes they spread and widen out a little, forming nodes like so many knots on a cord. Frequently the continuous line is interrupted by a gap or a series of gaps, so that instead of a uniform bank there are a number of huge circular or oval mounds. They consist generally of a main trunk, with a number of small subsidiary lateral branches running into it like the affluents of a river, and sometimes they have satellites attached to them in the shape of kame-like mounds. They are as sharply marked off from the adjoining plain on either side as a railway embankment is. In some cases, notably in Finland, they do not run in parallel lines, but vary in direction, sometimes even crossing each other, but in Sweden their direction is singularly parallel, as may be seen from the admirable maps published by the Swedish geologists, notably that of Törnebohm. The enormous size and cubical contents of these gigantic mounds can only be appreciated by those who have seen them on the spot and followed them for miles.

According to Erdmann, the well-known Upsala äs, which runs from the mouth of the Dalelf to Södertom, south of Stockholm, is about 200 kilometres long. The äs of Köping,

as far as it is at present traced, from Nyköping to the Dalelf, is about 240 kilometres in length. The äs of Enköping runs from near Trosa in Sudermannia to Loos in Helsingland, and is from 300 to 340 kilometres long, while the äs of Badelunda, running from Nyköping in Sudermannia to the parish of Rättvik in Dalecarlia, is about 300 kilometres long. According to Erdmann, the äsar west of the watershed between Lake Wenern and Lake Wettern run north-north-east to south-south-west, while east of that line they run from north-north-west to south-south-west.

Erdmann also gives the elevation to which some of the principal äsar have been traced. In Jemteland, north and north-west of Storojo, to 1,000 or 1,200 feet; in Herjeadal, near Hede, to 1,300 or 1,400 feet; in Dalecarlia, in the parishes of Malung and Idre, to between 1,000 and 1,300 feet; in the Government of Elfsborg, in Vestrogothland and east of Ulricehamm, to 1,100 feet; at Jönköping, in Smäland, near to Lake Almesäkra, to about 1,000 feet; but Törnebohm informed Mr. Geikie that in the northern parts of the country they occur at an elevation of 2,000 feet. Their height varies, the average being about 50 or 100 feet high, but in many places they run up to 100 metres or more, while they sometimes sink to twenty or thirty feet. Their breadth, too, varies, the normal breadth being from thirty to fifty paces, but in some cases, as at Upsala, where there is a spreading node, their breadth runs to 200 or 250 yards. From these facts the cubical contents of the äsar may be guessed. They are often somewhat wider and higher at their northern end, that is, at their inception, than farther on. In the low flat country their contour is very uniform, but in the upper and more hilly districts, where they chiefly abound, they have a tendency to become broken up into strings of separate mounds and kame-like masses. Their materials, in so far as they consist of boulders, have in every case where they have travelled, and we can trace the mother-rock *in situ*, moved from north to south, and never in the reverse direction.

One of their most important features, and one which has been a great deal too little noticed in the various theories which have been forthcoming to explain them, is the fact, that at lower levels than 300 feet they traverse the country

quite irrespective of its contour, going up hill and down hill, and athwart the natural drainage. Above that level they generally follow the valleys. On this point I will quote the language of a first-rate authority, Erdmann. After saying that they sometimes run along the valleys, sometimes on the mountain flanks, and sometimes on the plateaux, he adds (in italics) the words: "C'est ainsi qu'elles continuent leur cours lointain, franchissant les plateaux, les vallées, et les plaines, et ne semblant en aucune manière s'inquiéter des reliefs divers actuels du pays" (*Exposé*, etc., p. 41). This is a conclusion drawn from the Swedish äsar. The Finnish ones are quite as remarkable, traversing lakes and water-sheds without any hesitation. Another notable feature of the äsar is their great and numerous interruptions and want of continuity.

As I have said, a large portion of the äsar consist of masses of rounded stones of various sizes up to two feet in diameter. These rounded stones are not mixed with angular erratics. The latter when they occur, chiefly do so in the upper and more sandy and loamy layers, or scattered over the äsar-backs, nor, so far as I could observe, do these consist of stones of exceptional size, although they are frequently several cubic yards in bulk. The contents of the äsar are not sorted according to their size, and the stones generally lie with their longer axes parallel to the direction of the äs in which they are found. The beds of sand and the sandy äsar are in nearly all cases more or less stratified. They are frequently false-bedded, and the beds which show the false-bedding have their lines very pronounced, the angular wedges of sand and the lenticular masses being on a large scale and very marked. The uppermost layers of the äsar often consist of stiff blue clay or of finely sifted and laminated brick-earths, containing in places numbers of diatoms and marine shells, but never, so far as I know, fresh-water débris or land molluscs. These beds of brick-earth and clay occur only at the top of the äsar, where they are often intercalated with sand beds very irregularly disposed, just as they are in the beds of contorted drift in the Cromer cliffs, and they are generally continuous with the mantle of similar loam that covers the intervening country. I cannot follow Erdmann

and Geikie in separating these superficial layers in the äsar from the beds below. So far as I can judge (and here, again, the present condition of the cutting at Upsala is very pregnant with meaning), they pass continuously down into them, and are merely later phases of one deposit, just like the similar phases we see in the drift beds of East Anglia. Lyell, Murchison and others, who examined the äsar with care and skill, and whose judgment was in this case unwarped by *a priori* theories of their origin, treated the superficial beds containing marine shells as belonging to the same period as the lower beds which are barren and consist largely of boulders. Another feature of the äsar which is remarkable is the frequent passage in them of gravel into what is sometimes called true moraine matter, *i.e.*, heterogeneous matter, a passage which J. Geikie says may be traced not only on the sides of an äs, but also in the direction of its trend; the appearance of large and small angular fragments of rock in the heart of well-worn gravel, and the confused or dislocated or jumbled aspect of the bedding sometimes seen in the interior of an äs.

I described the American äsar or eskers at some length in my *Glacial Nightmare* (pp. 796, 797), and will only add a paragraph or two about them here. Prof. Stone (*Proc. Amer. Assoc. Adv. Soc.*, 1880, p. 510), who uses the terms esker and kame as synonymous, says of the American eskers: "The eskers of Maine are found at all elevations above the sea up to a height of 1,600 feet. They freely cross transverse hills 100 feet in height, even when a little deflection would give them a course through a valley. None surmount hills of over 200 feet. . . . In many instances eskers cross the beds of lakes, sometimes for many miles, and can be traced under water."

He says of a system of eskers he calls system "nine": "It lies wholly in a region of slates, conglomerates and limestones for nearly 100 miles. It then crosses the granitic range of mountains extending north-east from Mount Derent. For seven miles after entering the granitic area in Aurora the kame is composed almost wholly of slate, although the country on both sides is covered by granitic till, and shows multitudes of granite boulders and fragments. The granite

then begins to appear in the kame, and a few miles farther eastward kame plains are almost wholly composed of granite although the underlying rock is then a micaceous slate or schist, and the till shows that kind of rock freely. This shows that the kame materials were transported lengthways of the kames, and in general farther than the morainal material originally derived from the same locality. In other words, kame drift is glacial drift plus a variable amount of water drift. . . . The gravel is less water-worn and rounded in the eskers near their origin; this and the prevailing direction of the dip of the stratification of the esker material, as well as the expansion of many of the eskers into broad kame plains as they approach their southern termination, show their material was transported in a direction corresponding to their length."

Mr. Stone in a further paper in the *Journal of Geology* (i., p. 246) says that in the State of Maine along 200 miles of coast the eskers decrease in size as they proceed towards the sea, at the same time becoming increasingly discontinuous, until they terminate near the heads of fiords, without extending beneath the sea. For an account of the Irish eskers, I must refer to my *Glacial Nightmare*, pp. 793-794. I will only shortly supplement what I there said by an extract from an admirable paper by my versatile friend, Prof. Sollas:—

"The eskers in Ireland are usually confined to comparatively low ground, rarely more than 350 feet above sea-level, though some large isolated mounds are as much as 400 feet high. Their usual height above the surrounding country is seldom more than 60 feet.

"Their side-slopes are subject to great variation, but are often steep, making an angle of 35° with the horizon. Both sides may be almost equally inclined, but more commonly one side has a steep and the other a gentle slope.

"The run of an esker system may be with the slope of the ground, as at Ballyhannis, or against it, as at Portumna, or may change from one to the other, as the system of the Midlands.

"The structure of the Irish eskers differs greatly. In some places it is confused, consisting of a rubble of blocks of stone

only slightly water-worn, but without glacial scratches, piled together in disorder; in others it is beautifully stratified; coarse and fine gravel, sand, and occasionally clay, alternating in rapid succession. False bedding is common, and in many cases a rude tendency to so-called anticlinal structure is seen, *i.e.*, the beds of stratified material conform more or less closely to the slopes of the esker. . . . At the base of some eskers horizontally bedded sands and gravels are often met with, and in one instance these were seen to be beautifully ripple-marked" (*Trans. Roy. Dub. Soc.*, Series ii., v., pp. 818-819).

"In the Greenhills esker close to Timon Castle, Dublin, a particular bed of pebbles can be traced along a horizontal course for a considerable distance. It is then interrupted for about twenty feet; fragments of it, however, are clearly visible lying along an irregular curve beneath a mass of disordered sand and gravel. . . . Faults are not uncommon, contortions are frequently and reversed faults occasionally met with. . . . Large blocks of stone resting on the surface of an esker, and sometimes included within it, are frequently called attention to by officers of the survey. . . . Not infrequently a core of coarse and confusedly arranged material is found within a mantle of exquisitely stratified sand and gravel, which may be repeatedly faulted" (Sollas, *ibid.*, Series ii., v., p. 819).

Prof. Geikie identifies the Irish eskers and Scandinavian äsar with similar ridges occurring in the Scotch Lowlands and the lower reaches of Tweeddale and Nithsdale, where he describes them as ridges and mounds running in long, gently sinuous, and now and again more rapidly curving lines, following in their direction the trend of the valleys in which they occur, resting either on boulder clay or solid rock; and it is particularly worthy of note, he says, that the gravel and sand of which they are composed are strictly confined to the ridges and mounds—that is to say, their deposits do not spread out laterally upon the adjacent low ground. The steeper ridges consist chiefly of gravel, generally coarse and showing little or no trace of bedding. In many places they consist of tumultuous heaps of coarse gravel, shingle and water-worn boulders, or, as the case may be, of an agglomera-

tion of large blocks and angular and sub-angular débris mixed with earthy grit and sand. The abrupt embankments, on the other hand, are usually built up of finer gravel and sand which are often beautifully bedded (*Great Ice Age*, 3rd ed., pp. 168-169).

Let us now consider the theories which have been adopted to explain the äsar or eskers. In the very early days of the glacial fever, if I may coin an incongruous but not inappropriate phrase, when Agassiz reigned supreme, they were pronounced to be moraines. This conclusion is one of those which form the despair of rational science, for beyond the fact that they are heaped-up mounds of earth and sand and gravel, there does not seem to be a single feature about them resembling moraines. The stones they contain are rounded, water-worn boulders, in no way like glacier stones. Scratched stones and those with flat sides are, I believe, rarely, if ever, found in them; the beds of sand and clay they contain are sifted, very often stratified and cross-bedded, and absolutely different to the mixed-up, heterogeneous "muck" forming moraine-stuff. The shells they contain are marine shells, many of them perfect and of very delicate structure. Putting aside their contents, their other features are quite different to moraines. Terminal moraines, which are the only kind of moraines resembling the äsar in some phases of contour, are always planted athwart the line of march of the ice. The äsar on the contrary are all roughly parallel to the line in which the stones have moved and to the lines kept by the striæ on the rocks. If moraines at all, therefore the äsar must be either medial or lateral moraines, but ice-sheets could have no lateral or medial moraines. Who, again, has seen lateral or medial moraines made up of water-worn boulders and of stratified sands and brick-earth containing marine shells, or seen them ranged in a large series of parallel mounds with subsidiary branches, and with no high lands in between from which their contents could be derived?

Speaking of the so-called sand-äsar, which chiefly consist of sand and gravel mixed with a certain number of rolled boulders, Durocher says: "Entre ces collines et les moraines abandonnées par les glaciers, il y a des différences notables sous le rapport de la configuration extérieure, de la grosseur

des matériaux, de leur état de conservation et de leur distribution; les moraines n'ont pas la forme de terrasses ou de larges chaussées aplaties en haut, et à section trapézoïdale; il s'y trouve en général plus de gros fragments, les matériaux y sont anguleux, mélangés confusément, et ne présentent pas cette répartition par zones de sable et de cailloux que l'on remarque fréquemment dans le nord de l'Europe. D'ailleurs les blocs erratiques gigantesques, s'ils avaient été transportés par des glaciers, devraient se trouver exclusivement dans les accumulations de débris offrant les caractères des moraines, et l'on ne conçoit pas comment ils pourraient se trouver à la surface de dépôts qui présentent les caractères évidents de formations aqueuses. En outre, beaucoup de ces blocs proviennent de contrées basses qui ont été entièrement couvertes par les agents érosifs, et par suite, ils ne peuvent résulter de bouleversements ayant eu lieu au-dessus des glaciers que l'on suppose avoir strié la surface de ces collines: ils auraient alors l'origine qui leur a été attribuée par M. de Charpentier, savoir qu'ils auraient été arrachés par les glaciers à leur fond, et se seraient élevés progressivement à leur surface; mais cette opinion me parait difficile à admettre, d'autant plus que les très grands blocs qui gisent à la surface des dépôts de transport ne présentent pas de traces de frottement" (*Etudes sur les phénomènes erratiques de Scandinavie*, 1845, p. 60).

Berzelius, in a letter to Prof. Leonhard written as far back as 1841, says: "Agassiz' friend Desor visited us in September last year, and on seeing the immense boulder deposits which in this country are named äsar, stated without hesitation that these phenomena could not be explained by glaciers, and that they were not moraines" (*Quart. Journ. Geol. Soc.*, iii., p. 76).

Reclus, who, although not a professed geologist, has treated geological problems with intelligence in his great geographical work, is not less emphatic in his conclusion. Murchison and Verneuil and other "old masters" who examined the problem on the ground were of the same opinion. Nor do I know of any geologist who now maintains the view that the äsar are moraines.

If not moraines, what are the äsar? Hisinger suggested that they might be the remains of a gigantic denudation, the intervening and once continuous deposits of the same kind

having been swept away. This view, while it did not in any way explain the internal structure of the äsar, merely professed to explain their external shape and distribution. It has been completely analysed by Törnebohm, and shown to be quite untenable; nor do I know any one who now holds it, or who in fact professes to understand how such a denudation could come about. What kind of diurnal or other denuding agency would act in this way and sweep out so much of the beds and permit of these ramparts of soft materials remaining as they are when the rest of the beds were swept out? Whence could such an agency come? How could it work so as to move up and down the country irrespective of its contour and leave these mounds thus? Where has the débris of the gigantic denuding process gone to? How is it that the covering of the äsar, which is formed of finely levigated brick-earths, is also the covering of the intervening plains on either side? But I will not argue against a cause which has no defenders, nor kill again the corpse which Törnebohm slew.

The contour of their surface, the rounding and arrangement of the boulders in them, with their longer axes symmetrically placed parallel to the lines of the ramparts, the stratified sands and laminated clays, the current bedding, the presence of shells and microscopic protozoans, are all conclusive that the äsar are the result of aqueous action in some form or other; and Mr. James Geikie himself, who represents the high-water level of English and Scotch glacialism, says "all geologists admit that the äsar are in the main water-formed accumulations". Erdmann, Törnebohm, Nathorst, and all the other northern geologists known to me, are of the same opinion. When we come, however, to discuss the particular kind of aqueous agency to which the äsar may be assigned, and the method in which it worked, the unanimity at once ceases.

The superficial resemblance of the äsar, when drawn on a sheet of paper, to rivers with a main trunk and branching off into smaller affluents, perhaps first suggested the idea that they had something to do with rivers and river action, a view which has prevailed very considerably in text-books, but which seems to me to be absolutely untenable.

Two theories of the fluviatile origin of the äsar have been

propounded, one treating them as the result of sub-aerial rivers and the other of sub-glacial streams. I would first criticise the general theory of fluviatile origin.

In the first place, as we have seen, the äsar do not run along level surfaces nor along continuous slopes, but they frequently run up and down hill. Sometimes they are found at a height of 2,000 feet and sometimes only a few feet above the sea-level, and they run up and down the undulating country keeping the same general direction. Now whatever movements are possible with ice under certain conditions, by which it may be able to move up and down slightly undulating districts, and sometimes to creep up hill to a moderate extent, it would be an entirely new and surprising fact that water could do so, unless contained in a pipe and forced up by pressure behind. This is an initial difficulty of the first moment, and is in fact absolutely conclusive. Water, except in a pipe, cannot move contrary to gravity, cannot travel up and down hill or mount a slope, and it does not matter whether the water is in a channel open to the sky, or in a channel covered with an arched tunnel of ice. It is therefore impossible on this ground alone that the äsar could have been deposited by rivers of any kind, unless the contour of the country has entirely and radically changed since they were laid down.

This is by no means the only objection to the fluviatile theory of the äsar. Their shape, when viewed in section, is quite opposed to a fluviatile origin. Rivers which run very slowly and carry much mud, instead of depositing that mud entirely in deltas, sometimes, no doubt, raise their own beds, like the lower Rhine and some rivers of Eastern England do, and in this way make themselves solid aqueducts along which they flow. These solid aqueducts, however, have not the shape or contour of äsar, with their often steep and sharply inclined sides. This contrast in contour is even more marked in the heaps of débris which form the beds of sub-glacial streams. Nor can I see how rivers of any kind could raise their beds to the portentous height of the äsar and yet be so narrow. Rivers, again, must have banks, and if of fluviatile origin the äsar should have banks on either side while they have flat tops and no banks running along their crests. The

solid aqueducts we have experience of elsewhere are none of them very high, but are always breached and broken through after a time, where the river escapes and forms itself another channel, leaving the old bed meandering like a gigantic snake in the valley bottom. We cannot conceive such solid aqueducts remaining intact until they have been raised to a height of 300 or 400 feet.

Another difficulty presents itself when we compare the *contents* of the äsar with those of such river-channels as we can examine. Rivers which elevate their beds by gradual deposits are necessarily sluggish and slow-flowing rivers. When rapid, rivers become scouring agencies and not depositing ones. How is it possible to conceive of a sluggish river depositing these enormous masses of cannon-shot gravel, including great masses of rock, many yards in cubical contents —not of laying down a few yards of such gravel when there is an occasional rush in the stream, but a rampart a hundred miles in length and fifty yards high? The position is incredible. The Nile, the Rhine, the Indus, the Amazon, all these deposit beds, but they are beds of finely sifted mud. Again, in depositing stones, rapid rivers sift them according to their specific gravity, and do not mingle them higgledy-piggledy as they are mingled here. If it was a river that deposited these mountains of boulders and sand, etc., it must have been a very violent torrential river, and its force quite portentous along its whole course. If so, how is it that it did not scour and move away all the sand and brick-earth, and carry them down to its lower reaches, instead of laying them down along their whole course? All torrential rivers known to me have clean-washed, stony and gravelly beds, with deltas or reaches lower down, formed of the lighter materials of denudation.

But in bespeaking torrential rivers of this kind in Sweden we are postulating a virtual impossibility. The level of Sweden is too low and too flat to afford such rivers. To get rapid rivers we must have steep slopes in their beds. Of course we have a rapid enough flow at places like Trollhättan, on the Gota River, and in other gorges where we have rapids like those in the gorges of the Rhine, but there is no deposit like an äsar deposit in these gorges now. We cannot

conceive any deposit of any kind long remaining in such places, nor does it seem possible that these gorges existed when the äsar were made. Elsewhere than at these gorges the rivers of Sweden are quiet and slow-moving, and deposit not great masses of huge boulders, but sand and silt and mud. They must have been slower and less efficient as dynamical instruments when the level of the country was much lower, as apparently was the case in Sweden in so-called glacial times. Again, the rivers of Sweden naturally flow from west to east, or north-west to south-east, in channels in which they drain the upper plateau by running downhill to the sea, while, as I have said, the äsar run from north to south, right across the present river-channels and right across the lines of drainage of the country.

Again, rivers make deltas. When they have run their course, and get on to fairly level ground, they deposit fan-like stretches of mud and clay. There are no similar phenomena in the case of the äsar, which do not terminate as deltas at all, the flat spreads of gravel sometimes occurring in connection with them being torrential, and not like river deltas.

Rivers naturally have wider and wider channels as we move away from their sources to their mouths, and as their supply of water increases from their several feeders, and consequently as their loads of débris increase. This means that their beds become wider and deeper as we proceed downwards along their course. They are thus quite different to the more or less uniform ramparts called äsar, which chiefly change in bulk in the fact that they are bigger at their initial stage than later on.

It seems absolutely impossible to correlate the äsar of Dalecarlia and those of Finland, some of which actually cross one another, while others are united by cross pieces, with any river-beds, whether sub-aerial or sub-glacial. Again, rivers of any size generally contain fresh-water shells or other débris. The äsar, on the contrary, when they contain shells at all, contain *marine* shells only. Rivers do not deposit marine shells.

Lastly, we must not forget that although we are considering the äsar as substantive phenomena apart altogether from other deposits, it is only for convenience of treatment. We cannot, in fact, separate the äsar and their contents from the

sporadic and other deposits of the same kind occurring elsewhere. The äsar are only heaped-up ramparts of materials which occur in other areas in a less prominent fashion, in some cases as scattered boulders, in others as continuous beds of sand and gravel and brick-earth, or in terraces in valleys. Especially is this so with the brick-earth or loam which often forms their upper layers. This is really part of the continuous mantle of the country. Such different deposits occur virtually at all levels. How is river action to account for these complementary phenomena? Rivers cannot spread over a whole country so vast as Sweden. They would cease to be rivers, and would become quite transcendental, like Baron Munchausen's dreams, and if they did so they would interfere with each other's beds, and the ramparts would have been levelled down. It is clear that in finding an efficient cause for the äsar we must find one which will also explain the deposit of the drift occurring outside them. Apart from and altogether beyond these difficulties is the supreme meteorological objection as to whence the rainfall was to come to fill these stupendous rivers, running parallel to one another, quite near together, and forming such a web of rivers as was never seen elsewhere. Where is the gathering-ground and where are the watersheds which could produce such a congeries of rivers? This is an important matter for those among us who believe in inductive methods in science. It is apparently of no consequence to those geological alchemists who are continually engaged in extracting palm oil out of paving-stones. We cannot understand any meteorological or physical change which could supply the necessary rainfall for such rivers.

Therefore, on every possible ground known to me, it seems quite impossible to connect the äsar with river action. This is not my view only, it was also the view of my master, Murchison. He says: "However it may be argued that in mountainous tracts torrential rivers and their feeders may have descended as they do now, and may have produced rounded materials in valleys, the argument is, at all events, inapplicable to the formation of the Swedish äsar. These linear ridges have not only been accumulated in long *traînées* and lengthened mounds on terraces high above the valleys,

but offer appearances entirely unlike those produced by rivers." Some other very forcible and effective arguments against the fluvialite origin of eskers were given by the late Prof. Green (see *Glacial Nightmare*, p. 792).

Notwithstanding these very obvious objections to the fluvialite origin of the äsar, the view that they were in some way formed by rivers has very largely prevailed.

Struck by the branching lines formed by the äsar in which the continuous main trunks send out smaller affluent trunks, Mr. Törnebohm actually treats them as old river-beds, and has a most complicated theory by which he professes to explain the äsar, but in doing so presents us with many and more serious problems than the one he is solving.

According to this most extraordinary theory, the whole country traversed by the äsar was once choked (in some places to a depth of 180 feet at least) with a mantle of sand or mud. Through this sandy mantle rivers began to run, and first formed channels and then filled up these channels with the stratified deposits such as we find in the äsar. Lastly, some great denuding force swept away the whole of this mass of sand save only the river-courses which are now left as äsar. Can anything be more fantastic when tested by the actual facts? Whence was this mantle of sand derived? Whither has the product of denudation been carried? How could rivers be formed in spite of and at right angles to the natural drainage of the country? Where do we find any district traversed by a series of parallel rivers in this way, save in the case of the upper tributaries of some river with a vast watershed like the Upper Indus? Here, inasmuch as the subordinate äsar all join the main trunks on their eastern side, the rivers must have flowed from north-east to south-west, while the mountain chain of the Dovrefeld, the watershed of the country, runs north and south. How could rivers flow up and down, traversing great hollows like the Malar Sea? Where do we find rivers having continuous beds of this depth or character?

Dr. Hummel says of this theory that it is inconceivable that so much mud should have existed without leaving some trace behind, particularly as the form of the Swedish plateau

is such as would be likely to prevent its complete removal. Traces of the mud plain might be certainly expected to occur between the äsar or esker deposit and the underlying boulder clay, but though the contact of the two can be plainly seen there are no signs of mud, but on the contrary the boulder clay and the esker materials pass into each other (see Sollas, *Trans. of the Royal Dub. Soc.*, Series ii., v., p. 789).

Prof. James Geikie says: "Banks of gravel and sand no doubt accumulate in the beds of rivers, but if the rivers were to disappear, such banks would not form prominent ridges rising abruptly above the general level of the surrounding land. They would, moreover, coincide throughout any course with the lowest level of the valley, but our äsar, although they trend with the general inclination of the land, do not slavishly follow the line of lowest level, showing an independence of the minor features of the ground, sometimes winding along one side of a valley, and sometimes along the other" (*Great Ice Age*, p. 169).

In regard to Törnebohm's and similar views, Sollas has some pointed remarks. He asks how could a ridge of loose material have been deposited from water, as the eskers have been, and yet have acquired such steeply sloping sides? The only answer that can be given is that, during deposition, boundary walls must have existed by which it was held up. What then were these walls? Might they not have been a lateral extension of the esker itself, a lateral extension so wide as to constitute an alluvial plateau? An answer to this question in the affirmative has been given by at least one Irish geologist, Mr. M. Harrison (*Procs. of the Belfast Field Club*, Series ii., vol. i., p. 100). Törnebohm and others in Sweden had already proposed it. This view was rejected, *inter alia*, by Jukes (*Mems. Geol. Survey*, 98, 99, 108, 109, p. 8). In the first place, he says the existing drainage system of the country is not related to the eskers in a manner which would suggest such an explanation. In the next place, the structure of the eskers themselves is definitely opposed to it, since the stratified material of which they consist is frequently arranged so as to dip parallel to the sides of the ridge, and in addition, the separate ridges sometimes unite so as to

inclose deep hollows between them, which could not by any possibility have been excavated by a stream. Again, it may be asked, where have the denuded materials gone to? Sollas therefore urges that it is impossible to explain eskers as the denuded remains of a plateau.

While Törnebohm connected the äsar with ordinary sub-aerial streams, the great majority of glacialists have appealed to streams formed by glaciers or ice-sheets, considering that these would more or less meet the objections first urged to sub-aerial rivers. Hummel published his famous paper on the äsar in 1874. It is entitled "Om Rullstenbildungen," and is contained in the *Transactions of the Swedish Academy*. He first definitely connected the formation of äsar with the ice-sheet and its streams. He connected them with the ice itself by arguing, first, that their general direction was assimilated to that of the striæ on the rocks. Secondly, on the ground of the not infrequent interchange of material between esker material and boulder clay in the same system of eskers. Thirdly, because of the presence of angular erratics on the eskers, and also the presence of a covering to eskers not unlike terminal moraine-material; and, fourthly, the occurrence of marked dislocations in eskers.

On the other hand, he pointed out that while these characters connect the eskers with the ice, others connect them with running water, *i.e.*, the often beautifully stratified character of the kernel of the eskers, the frequent occurrence of false bedding, etc. Hence, he argued, they were due to running water acting over a long time with sufficient force, and a force acting at right angles to the direction of the esker. Nothing but a melting ice-sheet could, in his view, give these conditions (see Sollas, *op. cit.*, p. 790).

He then proceeds to argue that, as in modern glaciers, the water formerly flowed under the ice-sheets in tunnels or rows of tunnels.

"Given, he urges, the possibility of such action of the water on the bottom of the inland ice, and the explanation of the high-land eskers becomes comparatively simple. The ground moraine is exposed to the action of the flowing water; it becomes converted by degrees into a water-worn material, which is transported by the moving water to the glacier

tunnel, and is forcibly pressed in, until the tunnel is filled with it; the lateral streams are then compelled to form a new tunnel by the side of the old; the new one is in turn filled up and another excavated, and so on. The ice thus acts both by determining the movement of the water, and by affording a mould within which water-worn material is collected. The greater the thickness of the ice, and the greater the fall of the water, the larger the sub-glacial tunnels, and the greater the dimensions of the eskers. . . . The frequent deviation of the eskers from the direction of the glacial striation is due, according to Hummel, to the ramifications of the ice valleys, and from these ramifications also result esker spurs and knots. . . . The often obvious independence of the eskers, and the slope of the ground, their great and numerous interruptions, their isolated position, the appearance they often present of having been very rapidly heaped together—these facts, he urges, may be regarded as the effects of changes in, and on, the inland ice, and in the volume and velocity of the lateral streams" (Sollas, *op. cit.*, pp. 791, 792).

The views thus urged by Hummel did not become generally popular. They were followed by another theory which was largely adopted in this country and America. Prof. Geikie and Sollas both view it favourably, so do the more extravagant of the American glacialists like Salisbury. This theory was propounded by Holst, in 1876 and 1877, in his paper entitled "Om de Glaciala Rullstensasärne," published by the Stockholm Geological Society. Holst, while rejecting Törnebohm's mud-plain theory, points out that it had at least the merit of perceiving that the äsar required sustaining walls for their formation, and that these have since disappeared. He rejects Hummel's view of sub-glacial tunnels as the moulds of the äsar, on the main ground, I take it, that he could not explain how ground moraines could supply the gravel in the äsar, which is lifted up so high above the glacier bed. He urged, on the contrary, that these moulds were supra-glacial streams and not sub-glacial ones. He thus states his view. "On the melting of the inland ice streams formed on its surface, which worked back from the margin of the ice until they formed ice-cañons; at the same

time englacial drift (which is assumed to have existed in considerable quantity) was set free, worn by running water into rounded gravel, and sorted and arranged in layers on the bed of the stream. Thus the englacial fragments, which lay near the upper surface of the ice, formed the lower parts of the äs. On the final melting of the ice, the deposits in the river-bed remain behind as an äs. . . . The surface of äsar is sometimes covered with angular detritus. This, it is argued, is englacial drift, which was deposited in the last stages of the melting of the ice when the running water was diminishing in strength. Holst gives serious reasons for rejecting the formation of äsar from ground moraines as argued by Hummel. Thus, he argues, that as the äsar often run at a considerable height, to which running water could not lift their material, it was lifted up by the ice and from it carried down and heaped together in the äsar. The interruptions which so often occur in the course of an äs, he suggests, may have been caused by obstacles to the flow of the water. Thus a rapid accumulation of pebbles may have caused a diversion in the course of a river, so that instead of flowing in a cañon it would proceed over or under the ice" (Sollas, *op. cit.*, pp. 793, 794).

Salisbury's explanation of the American äsar or eskers closely follows Holst's. He argues that "super-glacial streams wear channels and valleys in the ice much as streams wear channels and valleys in the surface of the land. Other things being equal, valleys would be more easily excavated in ice than in rock, since ice is softer than rock, and since the streams would deepen their courses by melting their beds as well as by abrasion. Wherever there was rock débris upon the surface of the ice this would be slowly gathered into the channels of the streams by the movement of the water into them, as well as by the action of gravity, which would carry it down the ice-slopes bordering the valleys. If the ice which occupied the space of the valley-trough before the latter was excavated contained drift, such drift would accumulate at the bottom of the valley as the embedding ice was melting or worn away. Some part of the material, accumulated by one process or another in the super-glacial valley, would be removed by the water coursing through it. If the water was

unable to move all the material, the finer would go and the coarser would remain. In process of time, if the stream held its course without interruption, a more considerable filling of gravel and boulders might be made in the bottom of the super-glacial valley. Over and through this filling the water would move. Should the ice on which such a valley exists become stagnant . . . the ice beneath and around the valley would melt, and the sand, gravel and boulders accumulated in it would be let down, gradually and gently, upon the surface of the land immediately beneath the original channel in the ice. When the ice was gone, this ice-channel drift would appear as a ridge, coinciding in geographic position with the old super-glacial stream. If the super-glacial streams had tributaries, their valleys might have been partially filled with débris like that of the main stream, and when the ice had melted, the position of the tributaries would be likewise marked by ridges of assorted drift. Such ridges would stand in the same relation to the ridge of the main stream as the tributary rivers to their main one. Since the slope of the upper surface of the ice was towards its edge, the surface streams would have their courses determined thereby, and the courses of the resulting ridges would correspond in a general way with the course of the ice movement.

"The ice need not become stagnant before its melting in order that the material in the super-glacial valley should be deposited as a ridge on the land. Although overbalanced by melting, the ice had a forward motion during its recession in many if not in most places. In such cases there might be super-glacial streams where much gravel might accumulate if the surface or upper part of the ice furnished it. As the edge of the ice melted, the drift in the ice valleys would be delivered on the land in proper form as the ice melted beneath it, and by the progressive recession of the ice's edge the super-glacial drift would constitute a ridge of stratified sand and gravel. . . . The super-glacial valley might become very deep, even cutting through the ice. The stream would then rest on the land beneath the ice and flow in an ice-cañon, and would accumulate drift as before, only it would be recruited by débris from the ice on either hand, which would have a tendency to move inwards towards the

ice-cañon. This inward movement of the ice would be likely to disturb and contort the regular stratification of the material arranged by water alone. Such streams in ice-cañons would be a form of sub-glacial streams. The latter might build up similar long mounds, and if the stream was closely confined by the overarching ice it might not be able to abandon its bed even though this were higher than the land surface on either hand.

"Thus the sub-glacial stream might flow on a sub-glacial ridge in its channel because the only course accessible to it was along the crest of this ridge. In this case," we are told, "the ice would tend to press on on either side, and the more or less regular stratification produced by the stream in the deposits made in the channel would be disturbed and contorted" (Salisbury, *Annual Report State Geologist, New Jersey*, 1891, pp. 88-90).

Similar views are held by another great American glacialist, namely, Mr. W. Upham, who in his later writings, like Mr. J. Geikie, largely follows Holst. He does not, however, regard the larger angular erratics occasionally associated with eskers as having been left behind by a melting glacier but as dropped from floating ice. Like Holst, he supports the view that much of the glacial drift was carried within the substance of the ice, and he cites Bird's Hill esker, near Winnipeg, as proving that englacial drift was carried from a nearly level country to a height of more than 500 feet in the ice-sheet (Sollas, *op. cit.*, p. 80).

Sollas also speaks of an esker as representing a cast, as it were, of a glacier tunnel in gravel and sand, and presses the view that eskers must have been formed by ice, since some material must have furnished a support to the sides of the eskers during formation, and this could have been nothing else than ice.

Sollas says the chief difference which distinguishes an esker chain from a river system is its discontinuity, gaps of various length, from a few yards to several miles, breaking it up into a number of links of unequal size. These gaps, he suggests, may result from subsequent denudation in which liberated floods have breached them, or may mark stretches of glacial rivers where sediment was never deposited. He

confesses, however, that many important problems connected with the formation of eskers still wait for solution—such, in particular, as the remarkable manner in which they run up and down hills 100 to 200 feet in height.

Such are the main features of the two principal theories connecting the äsar with ice-streams, namely, that which connects them with sub-glacial and that which associates them with supra-glacial streams. Now, in regard to both these theories, it seems to be forgotten by every glacial geologist that a sub-glacial river, or a river flowing in a cañon or an ice-sheet, differs from other rivers only in the fact that in the one case it flows in an ice-tunnel, and that in the other the bed of the river is ice instead of being rock, or gravel, or sand. In every other respect it resembles an ordinary sub-aerial river in regard to its dynamical capacity and powers of portage, and is dominated by the same conditions; and every difficulty which has previously been pointed out in regard to the explanation of the äsar by fluviatile agency is as potent and conclusive against these postulated glacial or glacier rivers as it is against ordinary rivers. In addition they present special difficulties of their own which I criticised in my *Glacial Nightmare* (p. 814, etc.). Other objections remain, however, thus:

I am at a loss to know whence the water for these streams could be derived. The sub-glacial streams of glaciers are easy enough to explain. They are almost entirely due to the surface melting of the snow and ice which forms runlets on the glacier's back, and which fall down the crevasses in numerous cascades and accumulate in the streams at the base of the glacier; but in the postulated ice-sheets, a mile or two miles thick, there could, as we have seen, be no such crevasses; the tension required to crack such a mass of ice and make it gape would not be available, and if it were, such crevasses would close up again as they do in the case of glaciers when the ice reached level ground, over which it is said to have travelled for so many hundreds of miles. I cannot see, therefore, whence the sub-glacial rivers could be derived, for in the days of thick-ribbed ice everywhere, there could hardly be any melting of the substance of the glacier save on its surface, where it might be subject to ablation by direct solar radiation.

And suppose the water were obtained to form the rivers, these rivers can only have run down hill and along lines of least resistance, and could not make journeys over hill and dale as the capering ice-sheets are supposed to have done.

You do not in any way remove the difficulty of making water run up and down hill by covering your ice-streams with tunnels. In order that it should so run, the tunnels must be pipes gorged with water, and have water pressing upon them from behind, which is not postulated and would in fact be at issue with the very nature of ice-sheets.

No doubt tunnels exist in glaciers, and through them ice-streams flow, but the drainage from a modern glacier contending for half the year against the sun and warm winds must be excessive compared with any corresponding drainage from an ice-sheet in the ice age. How then are we to explain not merely ordinary ice-tunnels running up and down hill like a breakback railway, but extending for a hundred miles long in the same direction, 180 feet high, and in places only twenty or thirty paces wide, and cutting through the ice in a direction (as shown by the striæ on the rocks) very often different to that of the flow of the ice? Such tunnels seem to be quite out of the reach of any sub-glacial streams to make. If we could understand the making of the tunnels, how can we explain their being filled up to the enormous height mentioned by the débris which form the äsar? If the streams were sufficiently powerful to move the great erratics found in and on the äsar, and to cause the often tumultuous and confused character of their inner layers, they must have completely scoured their beds and swept away all soft materials like clay and sand, and must have also enlarged the tunnels to a great width by wearing away their sides.

Suppose the tunnels and the streams were available, how were the materials for making the äsar to be obtained? Suppose we grant the feasibility of the ground moraine theory as generally accepted by modern glacialists, and that we can understand a mass of ice under the pressure of many tons to the square inch "plucking" up pieces out of its own bed which it has previously crushed and broken—a view which I have previously criticised and shown to be fantastic—this would assist us very little in explaining what we want to explain.

If such a plucking process were possible anywhere, it must have been not where the ice-tunnel was, where there was no ice in contact with the ice-bed, but in the spaces intervening between these tunnels. It is there that the ground moraine ought to have accumulated and not in the bed of the sub-glacial streams; but it is exactly there that it has not accumulated; and one of the conspicuous features about the surface drift of Scandinavia is the accumulation of the great blocks on the äsar and their absence on the intervening plain; but even if the plucking process were possible on these intervening spaces, it could only supply a very small quantity of débris.

Directly a thin layer of such material was spread over the bed of the ice-sheet, how could any fresh materials be obtained by any process of plucking to build up any deposit to the height of some hundreds of feet? In order to pluck up stones the ice must have been in contact with its stony bed; and we utterly fail to understand how the plucking process of the American geologists could be carried on after that stony bed was protected by a mantle of soft materials. How could the ice go on plucking stones from beneath layers of drift forty, fifty or a hundred yards thick? We are here only considering the bed of the ice-sheet when the ice was in actual contact with it. The problem to be solved, however, is how débris could accumulate in the form of äsar when the ice was not in contact with its bed, but that bed was covered with a tunnel, and when the only dynamical agent available was the supposed sub-glacial stream. Where would the ice-stream get its ground moraine from under such conditions? It could not borrow it from the areas flanking it on either side, since there would be no lateral movement in the débris underlying the ice there, and these areas do not now contain materials like those forming the äsar. It could not get it from its own bed, for if ice can indulge in the process of "plucking," ice-*streams* cannot; and be it remembered the problem involves not merely securing a thin layer of sifted gravel and boulders in a sub-glacial stream, such as we find in living glacier streams, but the piling up of vast mounds more than a hundred feet high of sand and clay and other easily scoured materials.

There is no room for an appeal to the northern extremities

of the äsar as furnishing some clue to whence and how their materials were recruited from ground moraines. Äsar are not terminating banks whose earlier course is marked first by a long course where erosion and not deposition took place, and which became gradually higher as the supposed tunnels were more gorged with material. One of the features of the äsar is that they are generally highest where they start and become somewhat lower as they progress.

It must further be remembered that Hummel and the champions of his theory claim that the ice-streams rolled the boulders in the äsar into their present rounded and polished form. How could masses of gneiss and basalt be eroded and rounded when rolled not along a hard bed of rock but on beds of sand and clay containing a sprinkling of stones such as the äsar are? If such streams were capable of so rolling them, they must have scoured everything away. Hummel did not stop here, however. He went on to appeal to a fantastic capacity in ice for raising stones into its substance and lifting them up for hundreds of feet, and he thus explained the raising of the ground moraine in which he believed, and consisting so largely of loose sand and gravel, to the high levels where these last are now found.

I have already discussed *ad nauseam* this supposed capacity of ice to perform such extraordinary feats in mechanics. I will now only quote a sentence from Mr. James Geikie, which I think a very effective reply to the extraordinary notion. He says: "Some geologists have pictured to themselves a constant travelling upwards of the bottom moraine into the mass of the ice, and have imagined that in this way the ancient *mers de glace*, as they flowed on their way must have been more or less abundantly charged with sub-glacial detritus. But this notion is contradicted by all that is known of modern glacial action. The glaciers of Greenland, like those of the Alps, show included 'dirt lands,' but these consist of dirt and grit blown up by the wind upon the surface of the ice and subsequently buried under new falls of snow. The bottom moraine, as Drygalski remarks, travels persistently under the ice towards the ice-front, and is not carried up into the ice, so as to form included layers" (*Grönlands Gletscher und Inlandeis*, p. 50; *Great Ice Age*, p. 173, note).

It seems impossible, therefore, whichever way we view the problem, to have recourse to sub-glacial streams as an explanation of the äsar. There still remains the more popular notion of surface streams flowing on the back of the ice-sheet which originated with Holst.

It is quite true that when the sun beats upon the back of a glacier, or a warm wind does the same, it begins to melt and small streams begin to run on its surface, and after running a few yards they generally fall down a crevasse and disappear, thus recruiting the sub-glacial drainage of the glacier. If, as is generally postulated, however, the ice-sheets were of a portentous thickness reaching to two or three miles in depth, or anything like this, there could be no crevasses or ice cracks, the mass of ice being too great to give way to such cleavage, and there might consequently be considerable streams or ice-rivers running on the surface of the ice.

This, however, would be limited to the summer, when the melting could alone take place; and for the larger part of the year these same streams instead of containing water would be frozen tight and filled with frozen snow or *névé*, for the wintry conditions of such an ice age as is generally postulated would be quite intolerable. It must be remembered that Dr. Nansen, when he crossed the inland ice of Greenland, which is the nearest analogy we have to an ice-sheet, found no superficial ice-streams or rivers, but a continuous dome of ice and frozen snow. It is, therefore, very difficult to understand how Holst's portentous ice-cañons could be formed at all. They would have to be at least 200 feet deep with steep sides.

I have argued that water could not cut out fiords and cañons of this shape out of hard rocks, nor do I see how it could do it in the case of ice, especially when the flow of the ice was intermittent so that the channels would each winter be again choked with snow which would be hard frozen. I say that the proceeding seems quite fantastic.

Having secured the cañons, however, how were they to get filled up with drift, until they formed mounds of drift 180 feet high and running for 100 miles? There would be no recruiting of materials from projecting rocks, since the ice-sheet would smother all such under its enormous canopy. As Geikie very rightly says: "We have no grounds for sup-

posing that the old European ice-sheets would be charged with included morainic matter to a greater extent than the *mer de glace* of Arctic and Antarctic regions. Every one who has visited Greenland bears testimony to the remarkable freedom of inland ice from included morainic matter. When 'dirt' or stones are met with in the ice they are usually noted on account of their infrequency. Dr. Holst, who has himself studied the glacial phenomena of Greenland, has indeed described the occurrence of very considerable superficial moraines, but we must remember that these have been derived from *nunatakker* and the exposed rock surfaces overlooking the marginal areas of the inland ice" (*Great Ice Age*, p. 172).

It seems, therefore, hopeless to try and derive the contents of the äsar from superficial morainic matter of any kind. Holst does not in fact appeal to the supra-glacial source for his drift. He turns to another which seems much more transcendental, which he calls the inner moraine, the materials of which he derives from the higher rock ledges overflowed by the ice-fragments detached from the upper parts of ice-drowned mountains and cliffs and travelling forward imbedded in the mass of the *mer de glace*. This is surely quite arbitrary imagination. The pinnacles and rocks projecting above the ice get disintegrated in consequence of the intermittent action of frost and thawing, but below the ice the intermittent changes of temperature are impossible, except to a quite small extent. Such fragments again would be angular. How could they possibly be rolled into gravel and boulders on a bed like an äsar largely composed of sand and clay? If such a process were possible again, the upper ice layers of glaciers, whose movement is in all probability much faster than that of any *mer de glace* or ice-sheet, ought to show a large number of embedded stones instead of being as free from them as they are in Greenland, for instance. The supply of materials is therefore an unsolved crux to Dr. Holst and his followers. Let that pass, however.

Having secured a supply of stones, the glacialist who follows Dr. Holst has still to tell us how he proposes to build up his äsar with them. We are told that as the ice

melted, the débris it contained and which was derived as just mentioned would form a moraine in the bottom of the cañon —that is to say, the contents of the cañons would consist of the drift melted out of the ice which once filled them.

If so, why should there have been a selective choice by the ice-sheet of these particular channels in which the débris made its way to the surface and gradually piled up mounds and ramparts while the rest of the bed of the ice-sheet was virtually free from them? Surely there ought to have been a vast accumulation of the same material in the intervening spaces between the äsar. Nay, if we follow inductive methods, we ought to conclude that there must have been a much larger deposit in these intervening spaces where the ice lay thickest and longest, since there was nothing to remove the accumulated débris if such existed, while in the cañons occupied by streams there was a continual scouring process going on. The whole idea seems to me really childish. Again, the same difficulty seems to arise in this case also in understanding how the pebbles and boulders were rolled, as they are said to have been polished and rolled, when they were moving along a mere cushion of sand and clay.

I have argued this as if the depth of the äsar is a measure of the depth of the ice-cañons, but surely we should need ice-cañons of gigantic depth to supply äsar of 200 feet in height if their materials came from the sporadic contents of the ice which once occupied such a cañon.

Prof. Geikie points out other difficulties in Holst's theory. Thus he says: "As the materials continued to be dissolved out of the ice and to find their way down the slopes of the ice-valleys into the rivers, the bottoms of those valleys and the beds of the streams themselves would come to be protected from ablation. The ice of the water-divide and upper slopes of the valleys would by-and-by melt away more rapidly than those portions of the *mer de glace* which were deeply buried under rock débris. Thus, in time, the configuration of the surface would be so altered that the rivers would be compelled to desert their gravel beds, which by-and-by would themselves be converted into water-divides, while the sites of the former divides would be occupied by newer water-courses. In short, the tendency of the superficial water-flow would be rather

to distribute morainic material in irregular sheets over the surface of the ice than to arrange it in determinate linear courses; unless, indeed, we are to suppose that the superficial rivers succeeded in rapidly melting their way down to the bottom of the ice-sheet, and thus at an early period formed deep trenches into which was shot all the rock rubbish derived from the ice during its dissolution. If it be hard to conceive such conditions possible, it is not easier to see how sub-glacial river-beds filled with detritus to a depth of fifty or sixty feet more or less could retain their position and sink gradually down during the general ablation of the ice-sheet" (*Great Ice Age*, p. 174).

Prof. Sollas, who is a disciple of Holst, is nevertheless constrained to say: "From all I have seen of eskers, I should certainly say . . . that they have not been precipitated *en masse* from the bottom of sinking ice-cañons" (*op. cit.*, p. 819).

The arguments here used are amply confirmed by observations on such small ice-sheets as we can examine. Thus speaking of the supra-glacial streams of Spitzbergen, Garwood and Gregory say: "The channels both of the stream and its tributaries were generally quite free of débris. In fact all the supra-glacial streams flowed with such velocity that they kept their channels quite clear, except of an occasional boulder." In regard to sub-glacial streams they say: "Their velocity, so far as could be judged from the behaviour of the streams at the mouths of their tunnels, is probably even greater than that of the superficial streams. The latter flow in open channels, whereas the sub-glacial streams are forced through pipes, the size of which is no doubt kept at the minimum by the weight of ice upon the roof. It is only natural, therefore, that the power of the sub-glacial currents should be great, and it is not likely that deposits would be formed beneath them. We at least found no trace of gravel deposits resembling eskers formed either in sub-glacial or supra-glacial streams. The moraines of the Ivory glacier were in places kame-like in form, but they were certainly not formed sub-glacially" (*Quar. Journ. Geol. Soc.*, liv., p. 211).

Again, the same authors write: "As in the case of recent observers in Greenland (see *Journ. Geol.*, iv., p. 809), we

saw no sign of the formation of eskers in either supra-glacial or sub-glacial channels, or by streams flowing through ice-cañons. . . . None of the river-channels that we saw either in or on the ice contained much débris" (*op. cit.*, pp. 222-223).

While nothing like eskers is being at present formed in Greenland and other Arctic countries, it is a remarkable fact that mounds shaped like these hog backs and made of loose materials have been noticed in the Peruvian plains, where the intervention of ice-streams is of course impossible.

I have referred to the difficulty of assigning continuous mounds of the same general height and breadth, and extending over great distances, to the handiwork of rivers, whose beds naturally and necessarily grow wider as they march from their source to their outfall; but in the case of the Scandinavian äsar the sub-glacial theory presents an additional difficulty. According to the glacialists, the Scandinavian ice-sheet extended to the Carpathians and to Central Russia. How is it, then, that the äsar did not continue their strange march right away to these goals? Did the sub-glacial rivers stop short where the various Swedish äsar terminated their journey? If so, what became of their water? Was it frozen again? How was this? The farther south they travelled, the warmer must the climate have been under the same conditions, and consequently the more water must have flowed from the ice-sheet. Its drains, the sub-glacial rivers, must therefore have increased in volume instead of dying out, and surely when they reached the flat country of Poland and Russia, must have become more and more liable to deposit materials in their beds. How is it, then, that the äsar do not continue their march to Cracow? But apart from all this, let me repeat a question I have already asked, and which presents itself in a very grotesque fashion to a traveller who has crossed Central and Southern Sweden to and fro as I have. I wonder if the champions of sub-glacial rivers as the depositors of the äsar have ever drawn a series of contour-lines, say, from Dalecarlia to Malmö, and if so, how do they propose to explain the flow of any river, whether under an ice-arch or not, along such a course, across the undulating surface of Sweden? The same argument applies to America and Ireland.

There remains another element which I have not yet

considered, namely, the large, sometimes portentously large, angular and sub-angular blocks which occur in the surface layers of the äsar, and sometimes in large numbers on their backs, the äs near Gamla Upsala being a good example. Among the many boulders I noticed there, there was one whose cubical contents must have been thirty-six yards. Similar sub-angular and angular blocks have occurred in the deposit containing marine shells at Upsala. How are we to explain these blocks and their occurrence where they are found by any kind of fluvial action? Rivers must become desperate torrents, such as occur sometimes in the ravines of the Caucasus and the Western Himalayas, to move such stones at all; and how could such torrents arise under an ice-sheet, and how could these blocks be deposited by rivers in the midst of stratified sands with marine shells, and on the tops of the äsar? How comes it they are not found at the bottom, where the cannon-shot gravel so often occurs? When it is said they were transported by ice-rafts, how can ice-rafts be formed in either supra- or sub-glacial streams, or who ever saw such in any glacier stream, and if they did exist how could the great stones get on to their backs? If frozen to their under surface, the difficulty is still greater, for torrential rivers do not freeze into solid masses, nor does their ice-covering become attached to boulders which may be lying on their beds. So far as I know, rivers in Scandinavia do not now carry about and deposit such stones, except when very torrential or when there may be an occasional collapse in their banks; and if the glacial rivers derived them from disintegrated banks, it only removes the difficulty of their explanation one step further back. But those who appeal to ice in this fashion forget the relative age generally assigned to the surface beds of the äsar. The great majority of glacialists are emphatic about the surface beds of the äsar containing marine shells being post-glacial. If so, then we have a double crux, for not only have we to explain the existence of gigantic erratics far away from their parent beds, but to explain them and their portage when anything but a glacial climate is testified to as having existed by the mollusca occurring with them. It seems, therefore, impossible to appeal to ordinary rivers in order to explain these angular blocks; much more so is it difficult to

account for their portage by sub-glacial or supra-glacial rivers. In neither case do such rivers carry ice-rafts. Nor can we see how ice-rafts could arise in either of them. Ice-rafts are the broken pieces of ice which once covered sub-aerial rivers. The only ice which covers sub-glacial rivers is the roof of the tunnel of ice through which they flow, and which is a solid vault and not built up of pieces which could form ice-rafts; while supra-glacial rivers do not freeze on their surface, but cease to exist entirely in winter, for they are the result of the melting of the surface of the glaciers, and nothing more.

In every way we view the glacial river theory of the origin of the äsar it seems to collapse when analysed, and if I could be astonished at anything which the glacial geologists choose to formulate, I should be greatly surprised at its continued existence in their text-books. The time is assuredly coming when the reputations which have been built up on such science will as certainly collapse. No wonder that a desperate struggle is made to maintain them in some very limited quarters, and that a portentous silence prevails in others. It is in fact extraordinary how fantastic theories sometimes give way to common-sense when they happen to be inconsistent with other fantastic theories. Thus, in regard to a remarkable English esker, an extreme glacialist, Mr. Carvell Lewis, says:—

"I visited the so-called 'esker' in the park at Hunstanton. I saw in an open field a long hill of gravel resembling a kame in its narrow ridge-like form, but not resembling one in that the characteristic knobs and kettle holes were absent. It turned at right angles and continued as a narrow gravelly ridge in an east and west direction for one-third of a mile. An old battlemented square tower stands on part of this ridge, which is much steeper on the northern than on the southern slope. A sand-pit at the south end of the ridge showed it to consist at least in its upper part of water-worn coarse gravel and sand. This last looked like sea sand, not kame sand. Contortions with a little clay in them were seen in some parts of this cutting. I have never seen such contortions in kames, although they are common in marine drifts. Finally, in the sand I found many fragments of marine shells. Shells never occur in kames." Lewis concludes that the ridge is an old

sea-beach, and adds: "One part is parallel to the Wash, another part to the sea, and it stands just on the point between the two; possibly it was caused by the two currents, one of the Wash, and the other of the open sea. It is clearly marine, like the gravels of the neighbourhood. Is it a dune? or a bar? or a bank?" (*Glacial Geology of Great Britain*, pp. 340-341).

This is surely going back to Lyell's view, which was a good deal more sane than that now generally held, when he described the äsar of Sweden as having been produced under the sea, and speaks of the great blocks which lie on them as having evidently been lodged there when the ridges were submarine (*Geol. Proc.*, iii., pp. 342-343).

Whatever way we view the äsar or eskers it seems in fact impossible to explain them either by the action of ice or ice-rivers, or, as I believe, by any normal methods of nature's handiwork. I shall revert to them and the cause to which I assign them presently.

Closely connected with the äsar are the long trains or alignments of travelled blocks which are to a great extent parallel to each other, and which occur on both sides of the Atlantic. Perry says that of such boulder trains perhaps among the most remarkable are those which occur in Huntingdon, Vermont, and in Richmond, Massachusetts (*Proc. Bost. Soc. of Nat. Hist.*, xv., p. 62). I have referred to similar ones in Russia and Germany (see, *inter alia*, *Glacial Nightmare*, p. 734). Perry says that in America they can be traced for five, ten or fifteen miles. Hitchcock treated them in fact as äsar, and like the latter they run obliquely over high hills and through the deep valleys of Richmond. At Huntingdon, Vermont, is a train about two miles long and some forty or fifty rods wide, with a nearly north-south trend, and entirely made up of uncouth angular blocks strikingly different from any known rock in the district. They extend from one valley to another, passing obliquely across a steep intervening hill (*Proc. Bost. Soc. of Nat. Hist.*, xv., pp. 111-112). These alignments are assuredly äsar of another type, and seem like the latter to be quite inconsistent with ice portage or portage by glacial rivers. On modern glaciers we no doubt sometimes have medial and lateral moraines which trend away in lines from some decaying crag which projects above

the ice and supplies it with a constant shower of stones, but with ice-sheets such crags must have been all buried deep down under the ice. Neither could stones like these, many of them angular, have been accumulated by glacial rivers in their beds while the intervening districts were free from them, nor could they have been arranged in long alignments across hill and dale either by ice or ice-rivers.

Having dealt with the äsar, let us now turn to the so-called kames or turtle-backs and drumlins, those mounds of gravel and drift and of clay which are such a conspicuous feature in many of the mountain valleys where they open out into flat stretches, and in many cases have the appearance of artificial burial mounds. They are connected with the äsar and eskers in some cases by an intermediate class in which the continuous ramparts are really broken up into strings of such mounds, ranged in lines and forming no doubt a variety only, of the true äsar. Upham, an experienced observer of American so-called glacial phenomena, is of the same opinion. He describes the kames as long ridges or knots of interlocking short ridges and mounds, composed of obliquely bedded gravel and sand, which usually form prolonged series extending from north to south.

In the Connecticut and Merrimac valleys these deposits form ridges 100 or 200 feet high, which are clearly continuous in each case for more than twenty miles. Other series of kames of similar extent have been traced out in North-Eastern Massachusetts by Rev. G. F. Wright, of Andover, Mass., but their finest development yet found in America is in Maine, where Prof. George H. Stone, of Kents Hill, in that State, has traced twenty or more series which vary in length from five to 100 miles. The height of these ridges is usually less than 100 feet, to which they rise steeply, the slopes of their sides varying from 15° to 40°. Their material in some cases is almost entirely very coarse gravel with scarcely any layers of sand, so that they present no distinct stratification. I see no reason for separating them either in regard to origin or mode of formation from the true äsar. An esker or äs is, in fact, a glorified kame.

Again, a sharp division has been attempted to be drawn between kames and drumlins. Kames are so called by the

Scotch peasantry, and they thus name the turtle-backed mounds composed very largely of stratified gravel and sand; while the name drumlins has been applied to mounds of similar shape but composed almost entirely of unstratified clays. The latter were first separated and given the name of drumlins by the Rev. Maxwell Close in 1865. While the kames have been generally attributed to water-action, the drumlins have been treated as more directly the product of land-ice.

This supposed distinction in origin is founded, I believe, on a mistaken induction to which I have already referred, namely, that clay and sand must and do act in the same way when being carried or distributed by water, which in both cases it is supposed will lay them down in stratified layers. As a matter of fact, when very fine clay is continuously deposited from suspension in water, it does not arrange itself in laminæ and layers as when the water is flowing in intermittent streams or currents, but forms a perfectly continuous homogeneous paste which only becomes laminated in certain conditions. This has been frequently noticed, and the fact is a very good antidote to those people who see in every unstratified clay a glacial deposit. Thus Mr. Bulman says very truly that the absence of lamination in boulder clay is no proof that it was not deposited by water, since laminæ are caused either by an intermittent supply of sediment or the presence of flat plates of mica, and when the ice produces a continuous supply of rock flour all the year round, there is naturally no lamination, while flat plates of mica are also naturally absent (*Geol. Mag.*, ix., p. 410). Mr. Carvell Lewis speaks very emphatically on the same side. Thus he describes, near Bolton, a magnificent exposure of typical, tough, unstratified drift full of stratified boulders, the till being black and extremely hard and compact so that the stones must be hammered out of it, with no signs of stratification, and he suggests that it is a true moraine. He says, however, this very typical till was not glacier-made, but was made in water and deposited in a lake, as similar clay is now being made in the Swiss lakes and in the Arctic seas, and like the dense clay at Montreal, which contains scratched boulders and marine organisms together, being clearly of aqueous origin. Again, the same glacialist champion makes the same admission in still more emphatic language. Speaking of the

deposits at Ballygorey in Rosslare Harbour in Ireland, he says: "*The tough clay lies directly on irregular, jagged granite, flint and greenstone*, and contains beautifully glaciated stones of dark greenstone. Very finely glaciated boulders also lie upon the beach. In fact, the most beautifully glaciated boulders I have ever seen anywhere may be seen here lying at the foot of these clay cliffs, having clearly fallen out of them. *Yet this is a non-glaciated area. The underlying rock is not glaciated, and the tough clay is evidently marine and made at a time of submergence.* I am now convinced of what I suspected some time ago, that the most beautifully glaciated stones are to be found in tough clays of aqueous origin. . . .

"In many parts of England *south* of the glaciated area scratched stones occur when embedded in a stiff clay. Thus, at Rugby, 'well-striated' blocks of liassic limestone, chalk and flint occur in a clay which is a reformation of lias clay. . . .

"The upper and lower boulder clay of Lancashire in the Lowlands is also a moraine (?) deposit. The Norfolk and Cromer clays contain glaciated stones as does the York clay and the clay near Bolton Abbey, all of which are outside the glaciated area. The shells in the clay are often delicate and unrolled. Because there has been no attrition, the shells are preserved and the stones retain their delicate striæ. The stones have been dropped from floating cakes of ice or icebergs into a mud at the bottom of a quiet sea," and he speaks of tough, pure clays devoid of stratification occurring outside of glaciated areas as aqueous deposits, and says definitely: "Most boulder clays properly so called are of aqueous origin formed in water into which sub-glacial streams flowed" (*Glacial Geology of Great Britain*, pp. 158-159). The fact is, the want of stratification is no criterion whatever of ice-action.

A very good proof of the fact I am arguing about is the constant intercalation in the drift beds of homogeneous unstratified clays with stratified sands, the one lying on the top of the other without disturbance. They were due, no doubt, to currents of water from different directions having deposited their burden at different times, but in each case the depositing agent was unmistakably water.

It seems to me that it is from a neglect of this fact that

so many glacialists have drawn a sharp distinction between kames and drumlins. The two are really the result of the same agency, acting in the one case on sand in suspension and the other on clay; and they in fact pass into each other, it being difficult to pronounce in certain cases which is a drumlin and which is a kame.

Again, I cannot distinguish the contents and the internal structure of the kames and drumlins from the great mass of drift made of similar materials with which they are continuous, and which mantles the surrounding country continuously. The kames are merely a part of the stratified drift with a different surface contour, and the drumlins are the same as the sifted clays with a different contour. What has to be explained as peculiar to them is not so much the arrangement of their contents as their external form.

Let us examine this a little more closely.

Speaking of the great sheets and masses of distinctly water-worn material overlying the boulder clay, Mr. James Geikie thus describes them, and the passage is a remarkable specimen of limpid picturesque English. "These occur at all levels, from the coast up to a height of more than 1,500 feet above the sea. The most characteristic form assumed by them is that of rolling mounds, cones and ridges, all of which consist, for the most part, of gravel and sand. To such an extent, indeed, is this the case that the whole group is often spoken of as 'the sand and gravel series'. . . . As a rule these deposits are confined to the Lowland districts, where they appear to be distributed in a most arbitrary manner. Occasionally we may follow them for miles, when all at once they will die out, and then we may not meet with them again until we have passed into quite a different district. . . . The sands and gravels have a tendency to group themselves into mounds and winding ridges, which gives a hummocky and rapidly undulating outline to the ground. . . . It is most common to find mounds and ridges confusedly intermingled, crossing and recrossing each other at all angles, so as to enclose deep hollows and pits between. Seen from a dominant point, such an assemblage of kames looks like a tumbled sea—the ground now swelling into long undulations, now rising suddenly into beautiful peaks and cones, and anon curving up in sharp

ridges that often wheel suddenly round so as to enclose a lakelet of bright clear water. . . . The sharper ridges and peaked cones are chiefly made up of coarse gravel and shingle; the gentler undulations consist for the most part of fine sand and gravel. . . . Almost all the solitary mounds that I know of are made of fine sand" (*Great Ice Age*, pp. 180-182).

I have described the internal structure and the distribution of the kames in Great Britain at considerable length in my *Glacial Nightmare* (pp. 786-789), and will now merely supplement that description. The main facts to remember are that they are usually stratified and false-bedded, but in some cases "the coarser heaps of shingle do not exhibit traces of stratification, the stones being piled up in dire confusion. The stones, of whatever size, are almost invariably well rounded and water-worn. Sometimes, however, the deposits are not only unstratified but earthy, while the stones are angular or subangular, and such accumulations are associated with heaps of well water-worn gravel and hummocks of sand, in such a way as to show that they all form part and parcel of the same series. At levels above 900 feet and upwards the proportion of rolled stones gets smaller and that of rude angular moraine-like matter more frequent; occasionally till forms the nucleus of a kame, but sometimes the till has the appearance of having been intruded amongst the water-assorted deposits, the latter in this case being usually much disturbed." Embedded erratics are rare in kames, but frequently occur dotted over their tops and slopes. This position has certainly not been generally due to denudation. "Many of the mounds and hummocks upon which these erratics rest have evidently suffered little or no denudation since the period of their formation. The shape of the heaps and ridges is undoubtedly original, and we cannot but conclude," says Mr. J. Geikie, "that the erratics in question were dropped upon the surface just where we see them." Sometimes the bedding of the kames is much confused, twisted, folded and crumpled. Associated with the sand and gravel there occur in the kames sometimes deposits of clay and silt, usually finely laminated. "As we proceed from the low ground into the mountains, the gravel deposits become coarser, and the moraine-like matter assumes a more tumultuous aspect. Here we begin," says Geikie, "to meet with rude

accumulations of earthy and rocky detritus, mixed with coarse gravel and sand and large angular unpolished blocks. These deposits are sprinkled loosely over the mountain slopes, but in many places they assume a more or less distinct shape, so as often to form a striking object in a landscape, rising, as they sometimes do, in the throats of rugged mountain glens, into abrupt concentric ridges and mounds, the convex faces of which invariably look down the valleys. Frequently the whole bottom of a valley is covered with these deposits over wide areas, the ground presenting a very hummocky outline. Erratics of all shapes and sizes strew the surface of the cones and mounds and ridges, and are scattered over the bare rock itself. Excellent examples of these phenomena will be found in every glen in Scotland. . . . Even in such mountain valleys we shall find that the coarse moraines are often associated with heaps of sand and gravel which not infrequently cannot be distinguished from the similar mounds that occur in the kame areas of the Lowlands, while terraces of gravel and sand occur, ever and anon fringing the valley slopes often at considerable heights above the present streams. . . . It appears to hold generally true of all the larger areas of the kame deposits that they occur in valleys at or near where the rivers escape from the confined mountain glens or upland dales to enter upon the broad low grounds. The larger the drainage area, the greater the number of kames and the extent of the gravel beds" (*Great Ice Age*, pp. 182-185).

Kendall describes kames, when typically developed, as "ridges of gravel and sand taking a sinuous course over a country, and frequently disregarding the lesser, and even some of the greater, physical features. Thus they will sometimes run in straight lines, descending deep valleys, and crossing lofty hills. They frequently broaden out into a plexus of hills, enclosing within their ramifications small lakelets, or in some cases dry hollows, without any drainage outlet." He thus treats kame as an equivalent of esker.

In regard to the kames in Forfarshire, which he calls lateral mounds, Lyell says they generally increase in width and depth as they descend from the higher to the lower glens, attaining in the latter sometimes a thickness of 100 feet, and occasionally so great a breadth as merely to leave room for

the river to pass. The inferior part is always unstratified, consisting of mud and sand in which large, angular and rounded fragments of rocks are imbedded. These boulders are more and more rounded as their distance increases from the hills, whence they could have been detached, but they are more frequently flat-sided than pebbles which have been rounded by water, and they become more diversified in character by the junction of every tributary glen. In the upper part the mounds often consist of forty to eighty feet of the same materials as the lower, but regularly stratified (*Geol. Proc.*, iii., p. 339).

Jamieson describes the drift at Aberdeen as forming large swelling mounds and little hills on which a great part of the city and its suburbs is built. Again, it is met with tumbled up in tumultuous hillocks or longitudinal mounds parallel to the strike of the river. Its pebbles are never striated, and it is always highly water-rolled. From the peculiar position of the pebbles in the gravels, and from their false bedding, Jamieson argues that they had been deposited by rapidly flowing water flowing down the valley. They often have a crag and tail structure, the tail pointing seaward, long mounds having accumulated behind horns of rock. These and the dome-shaped tumuli and abrupt hillocks into which the gravel is occasionally thrown together, point, he argues, to a rapid current flowing down the valley as their cause.

Speaking of kames in America, Mr. H. L. Fairchild says: "The term is here used as designating deposits, chiefly sand and gravel, having a knot and basin topography. . . . The term esker (äsar or serpent kame) is employed to denote distinct ridges, chiefly gravel."

The American kames are described by Upham as "ridges showing an arched, irregular stratification, and composed of layers varying from sand to very coarse water-worn gravel with pebbles two or three feet in diameter. . . . They are found," he says, "in New Hampshire, sometimes extending continuously for many miles along the lowest part of a valley or elsewhere, short, and several parallel to each other; or in very irregular mounds and ridges with hollows enclosing small ponds. These ridges, or remnants of them, exist in

almost every river valley in the State. They are generally in the middle or lowest part of the valleys, but in the south-east of New Hampshire, in some parts of Maine, and in Eastern Massachusetts, where there are only scattered hills with slightly depressed valleys, they are found, though smaller than in the valleys, extending usually north and south, without special regard to the present water-courses.

"In the Merrimac valley the kames occur in a series extending for twenty miles. They consist of coarser materials than those last named, and contain fewer and only thin layers of sand. The stratification is sometimes indistinct, but whereever shown is anticlinal. Sometimes the rounded materials are succeeded by angular ones, consisting of angular blocks and earth-like till. Except that the blocks do not show any glaciation, they are plainly part of the same continuous kame, elsewhere composed of water-worn materials" (*Origin of the Kames and Eskers of New Hampshire*).

"Wherever deep valleys are found extending from north to south, kames seem to occur, oftener in these depressions than upon the intervening high land; but where no well-washed valleys exist, they are found threading their way in a south or south-south-east course among the scattered hills, often crossing them, with undulations of 100 or 200 feet" (*Proc. Am. Ass. of Science*, xxviii., pp. 7, 8).

Speaking of these kames, Dr. G. F. Wright says: "It is important to notice that the material in the ridges is not uniformly nor everywhere stratified. The ridges themselves are ordinarily composed of sand, gravel and pebbles, the latter from a few inches to two feet through, sometimes irregularly stratified. . . . At other times ten or fifteen feet or more in thickness will give no sign of stratification whatever. The top of the ridge is usually just wide enough for a footpath, and pebbles a foot or two in diameter dot its course at frequent intervals. . . . The fragments of rock in the ridges are nearly all somewhat rounded, and apparently water-worn. There are no striated stones" (*Proc. Bost. Nat. Hist. Soc.*, xix., p. 30). "Between Danvers and the sea the ridges are near the sea-level, and at several places sections show almost complete stratification. Above Beaver Bush Station the ground is higher, and the few places where I have found fresh sections

show little or no stratification" (*ibid.*, p. 54). "Between Conway and Madison there is a wonderful exhibition of very extensive moraines (! ! ! H.H.H.), full of coarse angular boulders of all sizes, very abundant, with none of the modifying action of water, in ridges, etc., just like kames in arrangement. They occupy the side of the valley, and pass by gradual transition to typical kames along the centre of the valley" (*ibid.*).

By drumlins the Americans mean "aggregations of till generally very compact, with a high proportion of stratified material of very symmetrical form and regular arrangement. The most typical are of elongate form, smooth and flowing contours and regular slopes; their end slopes are commonly gentler than their side slopes; their longer axes are approximately parallel with each other and correspond with the direction of the drift. In length they vary from one-eighth of a mile to two miles or more. In height they range up to 300 feet, though rarely more than seventy-five or 100 feet high." "They are known," says Salisbury, from whom I have taken this description, "as drumlins or drums, and have been called elliptical, lenticular or mammillary hills."

"There may be stratified drift within the body of the drumlins overlain and underlain by till. The core of the drumlins is sometimes stratified material" (*State Geologist, New Jersey*, pp. 71, 72).

Speaking of the drift deposits of North Germany, Geikie says: "Drums and drumlins appear to be of infrequent occurrence. The only place in which I have seen them is the island of Rügen in the Baltic, where they have been mapped by Prof. Rudolf Credner. In that region the drums frequently have a nucleus of rock (chalk)" (*Great Ice Age*, p. 432).

As I have said, I look upon drumlins as merely kames made of clay instead of sand and gravel. The fact that they occur in districts where the clays abound, and not in the districts perfectly continuous with them where the sands gradually displace the clays, is very eloquent on this point.

It seems perfectly plain from the features above referred to that the kames and drumlins are aqueous deposits, and could not have been deposited by ice, for they are composed of water-worn, water-sorted, stratified and cross-bedded layers. Nor could their external form have been given them by

ice, for their layers when examined prove to be concentric, and therefore to be parts of the original structure of the mounds.

Thus Jamieson speaks of the Aberdeenshire drift gravel as "being occasionally piled up in great undulating mounds and tumuli, forty to 100 feet high, the internal structure of which sometimes shows that their present form is not the result of denudation on what had formerly been a horizontally arranged deposit, but whose inward undulations conform to the exterior outline" (*op. cit.*, pp. 354, 355).

Goodchild says: "The external configuration of the drift mounds follows, in a general way, that of the rock surface on which they lie, and their component layers conform in their inclination, in a general way, both to the slopes of this core of rock and to the inclinations of the surface of the mound itself. . . . This is not confined to mounds of till, but is observable almost as frequently in well-stratified deposits, such as the mounds of sand and gravel. In eskers the fact of the correspondence between their external form and their structure has long been known." Kendall says of the great range of drift hills extending from Shelling Point *viâ* Kirk Bride and in a great sweep down to Ballaugh, in the Isle of Man, that "they present features characteristic of hills of deposition as distinguished from hills of denudation. . . . The hills have a billowy appearance, with very steep sides, they often inclose hollows without drainage outlets, and the bedding of the sand and gravel of which they are composed may be seen to be parallel to the slope of the hillsides. . . . In the town of Ramsey, along the back of Waterloo Road, is a long ridge of sand and gravel, displaying in some sections very markedly the phenomenon of bedding parallel to the surface" (*Glacial Geology of the Isle of Man*, pp. 9, 10).

Speaking of the Shelling sands, Kendall again says that "from Shelling Point they extend as a complex chain of hills of remarkably rounded form, each one, as a rule, standing very distinct from its neighbour, and revealing, when a chance cutting has been made, a *bedding parallel to the surface*" (*ibid.*, p. 17).

It is quite clear from this concentric arrangement of the layers that the external form of the kames was not super-

induced upon them after their materials were laid down, but was an original feature of them.

In the early days of the glacial fervour almost every kame or series of kames was treated as a moraine, and nearly all the so-called moraines referred to by Buckland in Wales and North Britain have nothing in common with moraines beyond the mound-like shape of their exterior.

It is quite plain from the concentric structure and rounded outlines above described, as well as from the fact that they are made up of rounded materials, sifted and stratified, and the way in which they occur, with their larger axes parallel with the valley, that the kames have nothing to do with moraines, which are formed of quite heterogeneous materials mixed like rubbish, and having only very slight and local signs of occasional stratification.

Every writer known to me now argues that the kames are, in fact, aqueous deposits. The great majority of the glacialists, however, connect them with their ice-sheets by means of the glacial streams to which they have such constant recourse. Thus Upham refers them to *glacial rivers*. "Sub-glacial rivers must," he argues, "have existed all through the ice age, which became greatly increased during the final melting of the ice-sheet. Other similar rivers flowed on the surface of the ice." He argues that the rapid waters of summer deposited the gravels, and the slower waters of winter the layers of sand, which would be very irregularly bedded. "On the general melting of the ice on each side of the rivers, the materials in their channels would be left in long ridges. . . . The occurrence of occasional angular boulders, enclosed in the surface of the kames, is readily explained, as the course of these rivers was probably, in most cases, beneath the glacier, and they would be dropped from the melting of the ice overhead. . . . The Connecticut valley for twenty-four miles is marked by a continuous kame (? an esker) 150 to 250 feet above the river, by which and its tributary streams it has frequently been cut through. . . . Its material is principally gravel, always water-worn; the largest pebbles one or two feet in diameter, with occasional layers of sand one or two feet thick. Occasional angular boulders occur. A section invariably shows an anticlinal stratification."

Nowhere, so far as I know, are such mounds being formed in river-beds; nor can I see how they could be. Many of them have their materials tossed about in a most tumultuous way, showing that the water which deposited them was torrential; but if so, and if it was running as a river, surely we should have had the whole of the sand and clay washed away, instead of having it deposited in layers and laminæ, and then overlaid by great erratics. How any one who looks at the great collections of kames strewing the embouchure of the Brora valley, or those of other Scotch flat valleys, where they are spread over the wide plain in scores without any regular alignment, can attribute them to river action, passes my comprehension.

As in the case of the rivers to which the eskers have been assigned, these kame rivers must have been able to run up and down hill, for, as Prof. Geikie is constrained to say: "There are many kames and mounds and elevated shelves, ledges and terraces of gravel and sand which have no apparent relation to valleys. Sometimes we find them ranging across wide open moorlands, at other times they occur upon watersheds, and not infrequently they can be traced up one valley over the dividing col into quite a different drainage area. Again, they occasionally appear fringing the slopes of hills and mountains, in positions and at elevations where it is plain that no mere river issuing from the end of a glacier could possibly have deposited them" (*op. cit.*, p. 226). He elsewhere refers to the "isolated mounds of sand that appear sporadically in the low grounds, often far removed from any water-course, and whose distribution seems to bear no recognisable relation to the configuration of the ground" (*ibid.*, p. 237). These instances are clearly unexplainable by any river action, and show to what straits those inquirers are driven who rely upon river action for their explanation.

The impossibility of correlating the appearance and contents of the kames with river deposits is not lessened but greatly increased when we make these rivers glacial rivers. This solution adds, *inter alia*, to the enormous difficulty of understanding how under any system of ice-sheets the materials of the kames could have been obtained at all. Since the rivers, if sub-glacial, must have flowed in vast tunnels, the ice could

not have obtained them from the bed of the ice-sheet any more than the expanse of the open dome of St. Paul's could gather up stones from its floor and deposit them again; and if the denudation of the floor was effected by the sub-glacial streams and rivers, how could they work through the padding of drift already there? The materials could not have been derived from the surface, for there were no exposed rocks to supply them; nor yet could the drains of the ice-sheet draw supplies from other parts of the ice-sheet on either hand, unless the ice-tunnels were continually shifting about; and if so, the mounds would have been destroyed and dispersed by the ice itself when it took the place of the stream.

Some of the champions of the ice monster seem to think that in some way or other sub-glacial streams because they are supposed to have flowed in tunnels could travel up and down hill irrespective of the contour of the country, and they thus explain the distribution of the kames and drumlins. Thus Mr. Kendall says, "as the sub-glacial rivers were flowing through *tubular channels*, they could flow uphill or downhill with almost equal facility" (*Glacial Geology of the Isle of Man*, p. 35).

This notion seems to me extraordinary. Of course if we have a reservoir of water, and we put a pipe into its side or at its base, the water will flow in that pipe up and down hill so long as we do not try to make it rise higher than the water in the reservoir, and so long therefore as we have a sufficient thrust from behind to keep it moving; but this thrust is absolutely essential. How are the ice-tunnels to be kept gorged with water? Whence is it to come? Again, water, however situated, will, whether driven from behind or flowing by its own impulse, follow the path of least resistance. Why should it proceed to carve itself tunnels running uphill instead of carving them running downhill? I cannot understand the process at all.

The view here urged by Mr. Kendall has been put in an even more transcendental way by Prof. Shaler.

Fairchild says of the kames in Western New York: "There can be no doubt that the greater part of the material has been derived from the Ontario excavation and rock degradation upon the north, and has been carried southward

uphill. It has been lifted hundreds of feet by either ice or water, or both combined. Upward currents probably do not exist in the body of the ice-sheet sufficient to lift the subglacial débris to such a height in so short a distance. Indeed, the material, if taken from the ground moraine, would require to be lifted far above its present height, as the fully rounded gravel and the large proportion of sand represent the wear of a considerable journey by stream transportation." He then goes on to quote approvingly an extraordinary theory of Prof. Shaler's, *viz.*, that in some manner the lifting of the sand and gravels may have been done by forceful upward currents of water at the ice-front impelled by the hydraulic pressure of water in the lofty ice-sheet to the northward ! ! ! "The chief and perhaps fatal objection," he says, "to this explanation is the extreme height of the Hopper range, which is 250 feet over the highest water-level" ("Kame Areas of Western New York," *Journ. of Geol.*, iv., p. 129, etc.).

A much more fatal objection is surely to understand how *water* contained in a saturated mass of ice can exercise any pressure at all upon an ice-stream below it. Whatever gravitating force water may have when it is in a reservoir is distributed in the interstices of the ice-mass when it is a case of the water saturating an ice-mass, and it cannot exercise pressure apart from the ice of which it in fact forms a part. Suggestions like those just cited are indeed the despairing resources of a lost cause.

In regard to the contents and structure of the kames, Goodchild also argues in his usual transcendental way. He first postulates that the ice of the ice-sheets had a vast quantity of débris of sand and stones scattered through its substance: a most arbitrary postulate, quite contrary to all our experience of ice-sheets and glaciers, in which it is only in very local and exceptional places that we find ice thus occupied, while generally it is quite free from any débris. He then enters into minute details in regard to the method in which ice gorged with débris in this way would act, which are quite as imaginary as Alice's discoveries in Wonderland. Thus, to quote one of his sentences: "The drift as we have it was *engendered beneath the ice after it came to a standstill*". I presume the meaning of this to be that the drift was deposited from the ice-sheet

by the gradually melted-out débris settling on its bed. If this were so, how did it become stratified, and especially concentrically stratified, and why was it deposited in turtle-shaped mounds? In certain places, again, the kames contain marine shells. This is surely a great puzzle to any one who treats them as of sub-glacial origin. Of this fact I will quote one case only.

"The fossiliferous Champlain clays," says Stone, "have hundreds of times been seen overlying the kames, and I have taken *mya*, *balanus*, and shells of other marine genera from the undisturbed clay found in the depression on a kame" (*Proc. Amer. Assoc. Adv. Soc.*, p. 518).

Upham, in regard to the drumlins, frankly confesses that "while some progress seems to be gained towards a knowledge of the manner and time of deposition of the drumlins, the question as to how the ice-sheet could amass these hills, and why they are distributed in abundance upon some districts, but are absent or represented only by a few examples upon other large areas, remains still unanswered" (*Proc. Bost. Soc. of Nat. Hist.*, xxiv., p. 242). And it surely will continue to be unanswered on the lines prescribed by Mr. Upham.

It seems perfectly plain that both kames and drumlins were the offspring of the same force, and that in whichever way we view them we shall fail to find in them anything like fluviatile deposits, whether the rivers appealed to be ordinary sub-aerial rivers, or rivers and streams connected with ice-sheets or glaciers. I cannot realise any method by which sub-glacial rivers, or any rivers at all with even slopes, could form great mounds, scores of feet high in their channels in some places and scour out their beds in others, nor how they should do this especially where the valleys spread out into great estuarine flats where the force of the water would be dissipated by being widely spread out. It seems to me that neither mediately nor immediately can we connect kames and drumlins with ice-sheets or with river action, and that we must turn from such causes as invalid to one which has no such disadvantages, but which explains all the kames equally well, whether situated in valleys or in the open country, namely, a rush of water sweeping over the country and breaking against mountains and obstacles with great force. Being thus stopped and thrust back,

and ebbing at such points with great force, the huge back-wash as the water returned down the valleys or met with serious obstacles would form mounds and ramparts of gravel and sand, which would naturally in many cases be stratified, and in others be tumultuously arranged. To this view I shall revert presently.

Let us now turn to another set of drift deposits.

I am quite willing to grant to the ultra-glacialists that their hypothetical ice-sheets are not consistent with the existence of medial or lateral moraines. These are only consistent with the existence of true glaciers moving down valleys flanked on each side by projecting crags, and in the days of ice-sheets when the whole country is supposed to have been swathed in snow there would be no such projecting crags available.

While this is so, the same reason does not apply to terminal moraines. A considerable part of the terminal moraines known to us come from beneath the glacier as well as from its back.

If the view, so often contested in these pages, that glaciers and ice-sheets were, and are, underlaid by what are called ground moraines, and that the great mass of the drift is really ground moraine, we ought to find clear and abundant footsteps of the great ice-sheets of the glacial age in correspondingly important terminal moraines. Each ice-sheet must have protruded a gigantic and portentous ground moraine marking its farthest extent, and perhaps others arranged concentrically marking the stages of its retirement. Now it is a very remarkable thing that no undisputed terminal moraines of the great ice-sheets seem to exist anywhere.

There are enormous collections of mounds in America and in Germany which have been treated by the ultra-glacialists as terminal moraines, but the fact of their being so has been sharply disputed, and it seems to me that they utterly fail to meet the conditions of true terminal moraines.

Those who hold them to be so are naturally the champions of ground moraines, and of the doctrine of englacial drift, doctrines involving, as I have tried to show, transcendental and not real qualities of ice. I have, therefore, a tremendous

a priori objection to these great moraines (so called) being treated as moraines at all.

Secondly, they seem to me to possess certain features which put them out of the category of moraines altogether. Where we can examine glaciers we find that the largest terminal moraines mark the ultimate terminus of the progress of the ice-mass. It is at the last halting-stage of the glacier that the greatest mass of débris has accumulated. It is there the great ice-brush has swept the largest surface possible of its débris, and piled it up in correspondingly large mounds. This we can see in the Alps, and in Norway this is the usual way with glaciers. Outside these terminal moraines we do not find what the Americans have called extra moraine drift—that is to say, precisely the same kind of materials spread out in a more or less continuous sheet, and having no actual frontier in the shape of great mounds at all, a kind of overlap of what are deemed the terminal moraines. I cannot understand how such an overlap could exist if these were true moraines. The drains of the ice-sheet, if they existed in any number, might lay down a quantity of washed and sorted materials in special channels, but the materials in question are neither washed and sorted, nor do they lie in separate beds like old river-channels. On the contrary, they are disposed in a continuous sheet forming a frill or apron to the great mounds, and extending for a long distance beyond them. How could ice-streams draining a mass of ice lay down such a continuous ribbon of deposit?

We are sometimes told that the ice-sheet melted very fast in consequence of some quite unknown agency, and that it was the floods caused by such melting which caused the dispersal of the extra morainic drift. I cannot understand the process. In order that there should be floods of this kind proceeding from the whole front of the ice-sheet, we must suppose that it had ceased to be a real ice-sheet altogether, and had become a glorified iceberg floating on a continuous layer of water, which seems to me almost physically impossible, and for which a great deal of evidence is needed which has not as yet been forthcoming. If we secured the water, however, such floods would surely have swept away all the sands and clays and left the boulders and the gravels clean washed.

Another theory to explain these fringes seems even more impossible. This accounts for them as the result of a previous glaciation. I do not see, however, how and why the ice-sheet of the previous glaciation should have left no terminal moraines and left only a fringe to the terminal moraine of the lesser glaciation. It must have been a larger ice-sheet than the one which deposited the inner and more northern mounds. Why then did it not deposit still larger mounds at its termination instead of only this more or less thin and continuous sheet, which the Americans call a fringe?

It seems to me that the presence of the extra morainic drift is very strong evidence against the so-called terminal moraines of the ice-sheet in America and in Germany being moraines at all.

Again, if they were moraines, it seems very curious that they should not be continued all round the outer edge of the ice-sheet. In America, however, they are only found in the Eastern States, while in the Western States there is nothing corresponding to them.

Thus Dr. Wright tells us that when he and Prof. Carvell Lewis were tracing the boundary of the glaciated area in North America, they found that upon crossing the Alleghanies and pursuing their investigations in the Mississippi valley they were compelled to abandon the notion that the terminal moraines could be traced across the continent, and were content to find marginal deposits more evenly spread over the country, ending in some cases in an extremely attenuated border (*Ice Age in America*, p. 121). Again, he says: "Through a portion of the southern boundary of the glaciated area in North America the glaciated deposits are so marked as to merit the name of *terminal moraines*. Through another portion that name is hardly applicable to anything near the glacial border" (*ibid.*, p. 123). Again: "The glacial margin on the Mississippi valley is not marked by such accumulations as characterise it east of the Alleghanies. The glacial deposits south of New England are truly phenomenal in their extent, and can with perfect propriety be called terminal moraines. West of the Hudson River it is difficult to trace a well-defined and continuous moraine along the extreme glacial boundary" (*ibid.*, pp. 176-178).

Chamberlin says that, "so far as yet ascertained in the interior, the drift limit is not marked by any such persistent ridge-like accumulations, but gradually dies away or is buried by later deposits, so that the precise limit of glacial advance is not easily determined. The only approach to an exception to this known to me is the case of the Kettle moraine in Wisconsin, where it lies on the border of the driftless area. Elsewhere around that area the drift thins out very gradually, so as to render the mapping of its margin a work of close inspection, and as the region presents no evidence of subsequent submergence, or any other special modifying agency, except the usual meteorological forces, this would seem to represent approximately the original form of deposit" (*op. cit.*, p. 28).

The American geologists, and especially Dr. Wright, have tried in many ingenious ways to get over the supreme difficulty involved in the facts just stated, and to find some reason for the discrimination shown by the great ice-sheet, which left great mounds at its supposed farther limit in some places, and left nothing of the kind elsewhere; but I altogether fail to see anything but special pleading in it all, and it, at all events, justifies us in the gravest *a priori* doubts as to these vast mounds having had anything to do with moraines at all.

This view is strengthened when we examine the internal structure of these supposed moraines. For a long time it was the fashion of glacialists to ignore the internal structure of the mounds, which they called moraines, altogether, and to give the name to any heaped-up rubbish or soft materials lying athwart the lines of drainage of a valley. This was the case very largely with Agassiz, Buckland, and even Lyell, who largely ignored the critical element of stratification or non-stratification of materials in discriminating what they called moraines. Hence every heap of rubbish, whatever its form or the arrangement of its contents, was pronounced a moraine, and hence the literature of glacial science is disfigured by many conclusions on the subject which are quite inept and really ridiculous.

Thus Buckland claims to have found a moraine "thirty feet high, stratified near the top to the depth of a few feet, but composed chiefly of unstratified gravel, inclosing frag-

mentary portions of a bed of laminated sand about three feet thick. Some of these fragments were in a vertical position, others were inclined, and the laminæ of which they were composed were, for the greater part, variously contorted" (*Geol. Procs.*, iii., p. 346).

Can anything be possibly more different to a moraine than this?

Again, in regard to another of Buckland's moraines, *viz.*, that on the elevated plain of Pentre Voelas, where are large deposits of unstratified detritus supposed to have been re-arranged by water, Macintosh says "this detritus contains no true rounded and striated blocks, nor rounded pebbles, nor a mixture of fragments of all forms and sizes such as usually belong to moraines. Snowdon, again, is twenty miles off, and there is no considerable height near, and Pentre Voelas is an open and widely extended plain, so it is impossible to see how and whence the necessary ice could have come." Another instance is between Pont-y-Gyffing and Capel Curig, and consists of what looks like a mound of gravel above certain dome-shaped hummocks in front of the confluence of the upper valley of the Llugwy with that of Nant-y-Gwryd. Macintosh, however, discovered that what appeared to be a mound of gravel was merely a low wavy ridge of schist, whose ragged and weathered surfaces seemed at a little distance as if covered by fragments, although, in fact, they are nearly bare, and he believes no gravel exists there.

Another of Buckland's moraines, says the same writer, is near the elevated lake of the Flynnon Llugwy, north of the road from Capel Curig to Bethesda. It consists of two principal streams of blocks, one extending down in a south-west direction from near the lower end of the lake for about two and a half miles, the other taking an opposite course, and reaching partly across the upper end of the valley, a distance of nearly half a mile. "The former," says Macintosh, "is marked by no glacial feature, and is chiefly remarkable for its length and conformity to the curves of the river, but the latter has at its farther extremity 400 or 500 detached piles of angular blocks about twenty feet in height. Near its point of commencement, also on the west side of the valley, it passes over two mounds, twenty or thirty feet high, covered with peat and

herbage, and a similar mound, somewhat larger and higher, occurs at the other end of the line, having also blocks on its top." Is this line a moraine?

Let us return, however. The stratified character of a large part of the so-called terminal moraines of America is a very marked feature of them. In all the descriptions I have seen of these so-called moraines, we have continual references to modified drift, or re-arranged and stratified drift, as being intercalated with the unstratified beds, and we have descriptions which recall the character of kames and other water-deposited mounds.

Thus, describing these accumulations, Upham speaks of "terminal moraines of the ice-sheet as occurring across New Jersey, where the margin of the glacial drift is marked by a series of very irregular hills *composed of till with occasional portions of modified drift*, which have been traced by Prof. Cook from the Delaware River a few miles above Easton by Hackettstown, Dover and Morristown to Perth Amboy. . . . For the twelve miles between Plainfield and Perth Amboy the terminal moraine is well known by the name of the Short Hills."

Again, speaking of Long Island, where the so-called moraine is very prominent, he says of Sandwich that the range there rises to 200 or 300 feet above the sea. "This portion," he adds, "consists *principally of modified drift often with numerous boulders embedded in it. Farther east it is partly modified drift.*"

It is a curious circumstance that we are further told no similar series of drift hills has been discovered to the north of these deposits in New England, and the only explanation which Mr. Upham can give is that the retreat of the ice was too rapid to permit of their accumulation (*op. cit.*, p. 13).

Chamberlin describes the Wisconsin Kettle Moraine as "an irregular series of drift ridges and hills of rapidly, but often very gracefully, undulating contour, consisting of rounded domes, conical peaks, winding and occasionally geniculated ridges, short, sharp spurs, mounds, knolls and hummocks, promiscuously arranged, accompanied by corresponding depressions that are even more striking in character. These depressions . . . are variously known as 'potash kettles,'

'pot holes,'. 'pots and kettles,' 'sinks,' etc. Those best known are circular in outline and symmetrical in form. . . . Occasionally they approach the form of a funnel or inverted bell, while the shallow ones are mere saucer-like hollows, and others are rudely oval, oblong, elliptical, or are extended into trough-like or even winding hollows, while irregular departures from all these forms are most common. In depth these cavities vary from the merest indentations of the surface to bowls sixty feet or more deep, while in the irregular forms the descent is not infrequently 100 feet or more. The slope of the side varies, but in the deeper ones reaches an angle of 30° or 35°, *i.e.*, is about as steep as the material will lie. The kettles seldom exceed 500 feet in diameter. A large number of small lakes without outlet dot the surface, and vary from ponds at the bottom of small kettles to lakes two or three miles in diameter. These are simply kettles on a large scale. Many of the small mounds look like inverted kettles.

"The range itself as distinguished from its superficial features is of composite character, being made up of a series of rudely parallel ridges which unite, interlock, separate, appear and disappear in an eccentric and intricate manner. Several of these subordinate ridges are often clearly discernible. Ridges running across the trend of the range, as well as traverse spurs extending out from it, are not uncommon features. The component ridges are themselves exceedingly irregular in height and breadth, being often much broken and interrupted" (*Wisconsin Kettle Moraine*, pp. 4-6).

"Clay, sand, gravel and boulders all occur prominently in the Kettle range, gravel being the most conspicuous element exposed to observation. The gravel and sand form an undulating sheet over the lower beds which are more uneven in surface than the upper one. The great core of the range consists of a confused commingling of clay, sand, gravel and boulders of the most pronounced type. There is every gradation, from boulders several feet in diameter down to the finest rock flour, and the erratics present all forms of angularity, from those scarcely abraded to thoroughly rounded boulders. The cobble stones are spherically rounded, rather than flat, as is common in beach gravel when the attrition is produced largely by sliding rather than rolling. *While the*

heart of the range is essentially unstratified, there is much stratified material intimately associated with it, a part of which, if my discriminations are correct, was formed simultaneously with the production of the unstratified portion. . . . The local overlying beds previously mentioned are obviously stratified, the bedding lines being often inclined, rather than horizontal, and frequently discordant, undulatory or irregular" (*ibid.*, pp. 7-8).

In an interesting paper by Mr. Leverett on the correlation of the so-called moraines of America with the raised beaches of Lake Erie, he shows how remarkably the two are connected, a fact surely incompatible with these so-called moraines being moraines at all. He does so in the case of three different beaches, namely, the Van West or Upper Beach, the Leipzig or Second Beach, and the Belmore Beach, each of which has its correlative moraine, and he says emphatically, "there seems to be no question that the moraine of the Eastern Lake Erie has in the Western Erie Basin a beach for its correlative". I may add that the description given by Mr. Leverett of these so-called moraines does not seem to me consistent with their being anything of the kind. He says: "Instead of a uniform deposit of till at the surface there is a variety of formations remarkable for the abruptness of their alternations. In one knoll a fine sand may occur while its neighbours are composed of clay, or a portion of a knoll may be sand and the remainder clay, the whole being moulded together in a symmetrical knoll, like the gravel and till in ordinary kames. One of these so-called moraines," he says, "was one of the first moraines to be described on the American continent, having been so described by Mr. Gilbert in 1871" (*Wisconsin Acad. of Sciences*, viii., p. 233, etc.).

The theory that these great series of mounds described by the American geologists are moraines, seems to me to be based entirely, or almost entirely, on their external contour. Directly we get information about their internal contents, we discover that they are to a large extent formed not of unsorted moraine matter at all, but of materials deposited by water, and consisting of sorted and sifted gravels and sands, arranged in concentric layers and showing signs of cross-bedding. They are generally, in fact, described as if they were glorified kames which are avowedly aqueous deposits, and I confess I cannot

see how they are to be treated as anything else. The view that they are not true moraines has been urged by more than one distinguished observer. Thus Dana says emphatically that there are no lateral moraines of the glacial era in New England. He also says that no distinct terminal moraines of the glacial era have been observed in New England, and he actually accounts for this by the glacier having terminated in the ocean on the east-south-east and south, and in the further fact that the melting of the Champlain era, *i.e.*, of the era of great floods, took place over too vast an area for the formation of proper terminal moraines, and he argues that the universal covering of drift on Long Island was deposited during the early Champlain (*i.e.*, the Flood) period (see Dana, *The Glacial and Champlain Eras in New England*).

Sir Wm. Dawson says: "I may be excused for continuing to regard the supposed terminal moraines of great continental glaciers as nothing but the southern limit of the ice-drift *of a period of submergence.* In such a period the southern margin of an ice-laden sea, where its floe-ice and bergs grounded, or where its ice was rapidly melted by water, and where consequently its burden of boulders and other débris was deposited, would necessarily present the aspect of a moraine, which by the long continuance of such conditions might assume gigantic dimensions" (*Report Brit. Ass.*, 1886, p. 40).

Again, the same writer says: "The great Missouri Coteau to which Dr. G. M. Dawson first directed prominent attention as a glacial feature, and which fringes the margin of the third plateau, about 400 miles west of Winnipeg, is now known to be continuous with similar ridges extending southwards into the United States, and eastwards towards the Atlantic, and which have been described as the terminal moraine of a great continental glacier. In the western plains, however, where it has its greatest development, it cannot be explained in this way, but must mark the margin of an ancient glacial sea, or at least of that deeper portion of such sea in which heavy ice could float, while in its upper portion it shows evidence of having been, in the later periods of its formation, an actual water margin" (*Ice Age in Canada*, pp. 123, 124).

Again, Prof. G. M. Dawson says: "The great drift ridge

of the Missouri Coteau at first sight resembles a gigantic glacier moraine; and marking its course in the map it might be argued that the nearly parallel line of elevations, of which Turtle Mountain forms one, are remnants of a second line of moraine produced as a feebler effort by the retiring ice-sheet". After arguing against this notion, he adds: "In attributing the glacial phenomena of the great plain to the action of floating ice, I find myself in accord with Dr. Hector, who has studied a great part of the basin of the Saskatchewan, and also as far as I can judge from his reports with Dr. Hayden, who, more than any other geologist, has had the opportunity of becoming familiar with all parts of the Western States" (*Quar. Journ. Geol. Soc.*, 1875, pp. 619-620). He then goes on to postulate a great submergence, the result of which was a corresponding large sea on which icebergs floated. In regard to this theory of a permanent submergence, I can only partially agree with my two friends just quoted, but in attributing the so-called terminal moraines of America to the deposit of water, and making them gigantic water margins, I am completely at one with both of them. Let us now pass on and turn to Germany.

Von Cotta, in his geological sketches of the neighbourhood of Dresden (1845), was the first to call attention to what Jentzsch and others call the *Diluvialhügel* there. Von Cotta says: "It is as we approach the hilly country that these drift mounds begin to appear, the country to the north being very flat. They consist of accumulations of small boulders, while in the neighbouring low beds there is only sand and gravel with occasional boulders. South and west of Rossendorf, between Dresden and Schmiedefeld, large numbers of these mounds occur from twenty to 100 feet high. Near Knichlen is a small hill of the same character, from 100 to 130 feet high." Von Cotta mentions others in other places as the Spitsberg, near Rodeberg, the little hills south of Gommalitz and Weixdorf, the long mound between Jessen and Oberaer, and others between Taschendorf and Weinböhla, the windmill hill near Gleiva, and the hills near Quees, Neschintz, Blooschütz, Salzförstchen, Kleinwelka, Sonnenberg, Bodewitz, Sinknitz, Lomnitz, Tauchritz and Penzig. A conspicuous one is the Mühlberg, near Dörfel, with its

eastern neighbours. Others between Königsvazte and Grostewitz, near Grossdubran, between Rochel and Gröditz, and between Neudorf and Burkersdorf.

Von Cotta says of these mounds: "Aus allem geht hervor, dass diese Diluvialgebilde ihre Enstehung einer sehr allgemeinen aber unruhigen *Wasserbedeckung* verdanken. Die Geschiebe, der Sand under der sandige Lehm, im Gerade so ueber der Gegend vertheilt, wie man die Vertheilung durch ein bewegtes, ueber einer unebene Fläche ausgebildetes Meer, erwarten kann. Wo Strömmungen waren, wurde alles weggespült." Writing in 1872, Jentzsch describes these mounds at some length, and suggests two possible causes for them. He urges they are remains of denuded stretches of sand beds. In favour of this he claims that their shape points to their having been shaped by the sea, and he thus explains also the presence on their backs of the northern erratics. The second cause to which he inclines is that they represent ancient dunes, on which the sand has accumulated upon deposits of mud left by the retiring sea (*Die Quartar der Gegend von Dresden*, pp. 9, 10).

To our English glacialists these mounds represent not dunes or diluvial beds as they did to the earlier German explorer, but moraines, and I will quote some paragraphs about them from Dr. Geikie, italicising the phrases which to me seem conclusive admissions that they are quite different to any true moraines. Thus he describes what he calls a remarkable series of terminal moraines confined to the Baltic coast, and marking the extent of the upper boulder-clay glacier. "They have been described by various authors as circling round the southern coast of Norway, then sweeping south-east by east across the province of Gothland in Sweden, passing through the lower ends of Lakes Wener and Wetter, while similar moraines mark the terminal front of the inland ice in Finland." Geikie speaks of at least two parallel frontal moraines passing inland from Hango Head on the Gulf of Finland. These terminal moraines have been traced north as far as Lake Tuoppa and the shores of the Gulf Kantalahti (White Sea). South of the Baltic, Geikie describes the Baltic ridge as extending from east to west through Eastern Holstein, Mecklenburg-Strelitz, Uckermark,

Neumark, Southern Pomerania and the higher parts of east and west Prussia, and consisting of a belt of land on which lakes and lakelets are abundantly developed. It is a true *paysage morainique.* It is characterised by an irregular rolling surface, mounds, hummocks and ridges of shingle, gravel and sand often associated with boulder clay. In the depressions enclosed by those interosculating and irregularly distributed hillocks are lakes, bogs and marshes innumerable. "The deposits," says Geikie, "are recognised by the German geologists as terminal moraines." He goes on, however, to speak of the confused bedding of the hillocks and ridges, of their contortions and disturbances, signs consistent only with water deposit.

"In their form, grouping and structure the hummocks and banks of gravel and sand which I examined in Uckermark and Mecklenburg," he says, "often *recalled to me the kames of Scotland.*" The kames of Scotland are surely unmistakable aqueous deposits, and not moraines.

Geikie then goes on to describe certain ridges composed for the most part of boulders, "hence German geologists call them 'Geschiebe wälle' (boulder walls). Sand and clay, however," says Geikie, "are not infrequently present, and occasionally the whole ridge appears to consist of *bedded gravel and sand sprinkled atop with erratics.*"

These are surely curious moraines, and it adds to the curiosity of their being described as such when we are told that immediately *south* of them the sand and gravel are spread out in wide sheets, and not heaped into hillocks and ridges. In Finland also the moraines are described as sorted and made up in places of water-worn materials.

The so-called Baltic ridge is roughly concentric with the southern coast of the Baltic, and is separated from it by a band of low land, covered chiefly with boulder clay, overlaid in places by interrupted sheets of sand and alluvium, and traversed here and there by well-marked äsar. Geikie admits that water has played an important *rôle* in the formation of these deposits, since they are so largely composed of sand and gravel. "In certain places they consist of horizontally disposed beds abruptly truncated. Thus some of the rolling hills in the neighbourhood of Angermünde (Uckermark)

appear to consist largely of horizontally, finely laminated sand, often showing diagonal bedding. Amongst this sand layers of clay and bands of gravel and now and again stones and large blocks occur, many of which are more or less water-worn. The blocks seem to be more abundant on the tops of the sand hills. These water-arranged deposits not infrequently inosculate with boulder clay, showing both were accumulated side by side." Geikie refers to the confusion and disturbances the bedded deposits sometimes exhibit, and he actually refers to the so-called morainic mounds as the German kames.

The modern glacialists among German geologists in order to explain the drifts of their country are constrained to argue that their great ice-sheet passed over the Baltic ridge and flowed south to the valley of the Elbe, where the deposits of boulder clay are so thick. To which Mr. Geikie reasonably replies "that the advance of a *mer de glace* across the Baltic ridge must have resulted in the demolition of mounds and hummocks and in the wholesale redistribution of their materials". This is quite true, but it leaves unaccounted for all the phenomena of this very wide extra morainic fringe in Germany with the great quantity of erratics it contains. The so-called upper boulder clay of Germany and Finland stretches "far beyond the limits of the Baltic ridge," and Geikie says "that the Baltic glacier, which deposited it, must have greatly exceeded the glacier which left its terminal moraines in South Sweden, East Jutland, and the Baltic provinces of Germany and Finland". If this was so, where are the terminal moraines of this gigantic ice-sheet which laid down the great masses of boulder clay in the Elbe valley, and the two boulder clays said to exist south of the ridge?

Let me now quote some more detached statements of the same most ingenious writer. "It may well be doubted," says Geikie, "whether the superficial débris (in Germany) ever reached the terminal front of the ice-sheet in sufficient bulk to form conspicuous moraines. It seems most probable that the terminal moraines of the great ice would consist of low banks of boulder clay, and hillocks and hummocks of sand and gravel or 'kames,' the latter, perhaps, strongly predominating, and containing here and there larger and smaller

angular erratics which had travelled on the surface of the ice. . . . The extreme limits reached by the ice are determined rather by the occasional presence of rock striæ and *roches moutonnées* of boulder clay, northern erratics, and gravel and sand, of northern materials in sheets or in mounds, than by recognisable terminal moraines" (*Great Ice Age*, p. 437).

Geikie, as a partial explanation of the rarity of such moraines says, as it seems to me, quite arbitrarily: "It is certain that the whole region in question has been considerably modified by subsequent denudation, and to a large extent is now concealed under deposits belonging to later stages of the pleistocene period" (*ibid.*, p. 437). For this I know of no evidence whatever.

He himself speaks of the dove-tailing and interosculation of boulder clay with aqueous deposits in North Germany, thus admitting the contemporaneousness of the stratified and unstratified beds; and also speaks of the ice, which he makes jointly with water the cause of the phenomena as having been tunnelled by rapid streams and rivers, and as here and there arching over *sub-glacial lakes* (*ibid.*, p. 435). *Sub-glacial lakes* surely involves another advance into the transcendental cloudland of modern geology.

Geikie allows the existence of bedded deposits in the boulder clays of the peripheral region of North Germany, and the occasional silty and uncompressed character of the clays themselves. The drift in Holstein, Mecklenburg and Pomerania, which is of a depth of 100 to 150 metres, and that of Hanover, Mark Brandenburg and Saxony, which is of still greater depth, he says is *composed of water-formed beds* (*ibid.*, p. 434).

The beds he calls aqueous deposits, *i.e.*, the stratified beds, are infrequent where the boulder clays occur. "They are either wanting or only occur sporadically in thin irregular beds in the high grounds of Northern Europe generally. Farther south they acquire more importance, until in the peripheral regions of the drift-covered track they equal and even surpass the boulder clays in prominence. These last presently cease to appear, and the whole bulk of the 'northern drift formation,' along its southern margin, appears to consist of aqueous accumulations alone" (*ibid.*, p. 434). This seems to point to the clays having, by their greater gravity, been

deposited first, while the sands were carried farther, and the loess farther still, a sorting process only possible with water.

It would seem, therefore, that the so-called terminal moraines of North Germany, like those of North America, are not moraines at all, but are what the old masters long ago described them—the margins of some widespread movement of water.

We will now leave these great continental areas and come home to our small island. Numerous attempts have been made to discover the terminal moraines of the so-called Scandinavian ice-sheet which is supposed to have invaded Eastern England, and brought thither the Scandinavian boulders which have occurred in the coast districts of South Yorkshire, of Lincolnshire and North Norfolk. It is in the first place remarkable that where these Scandinavian boulders most abound, namely, in Lincolnshire, there are the fewest signs of anything in the shape of mounds, etc., which simulating their form might be taken for moraines, almost the whole surface being flat and smooth. Mr. Carvell Lewis tries to explain the anomaly in a characteristic way. He claims that the drift in Lincolnshire is an extra-morainic fringe, and that the far-travelled erratics (which, by the way, are all very much rounded) rode on the top or on the upper layers of the ice, and that the local materials were below ! ! ! The latter, he adds, were left in the terminal moraine dropped in the sea outside of Lincolnshire, while the former were carried on icebergs which stranded here with their freight. Smaller icebergs, freighted with small boulders, got into the estuaries as far in, for instance, as Welton. All the largest boulders were stranded on the main shore at Louth (*op. cit.*, pp. 234, 235). How there could be icebergs or a sea to float them when the North Sea was occupied by an ice-sheet, I do not know; nor do I know of any evidence of any kind in favour of the conclusion that there are submerged moraines or anything like them off the coast of Lincolnshire.

Nor are the Scandinavian boulders such as are carried on the top of glaciers, for they are not great angular erratics, but small and very much rounded or kidney-shaped boulders.

In regard to the surface beds of Lincolnshire, Mr. Lewis, again, has one or two admissions bearing on our present

contention. Speaking of the Hessle clay about Louth, in Lincolnshire, he says "it is an aqueous clay"; and he adds, "no moraine mounds, nor trace of moraine character, are seen in this clay". Of the mounds at Withgall, made up of decomposed and fragmentary chalk, he says: "Here, then, the chalky boulder clay has its origin, being here made altogether of chalk, clay being absent. *Is it not due to torrents of water rushing westwards and bearing cakes of ice?*"

Speaking of a flint gravel thirty feet deep at Welton, he says: "It is marked by layers of clay and sand as if marine," and asks if "it is not due to the bursting of a marginal lake," adding, "this is the focus of the district where the Scandinavian boulders most abound".

"Above Louth," says Lewis, "is a very low, flat ridge of local character. . . . It has none of the characters of a moraine, but has the shape of a sub-aqueous bar or mud bank. It is not over twenty feet high at the highest part."

Let us now turn to Yorkshire, where the chief proofs of the existence of terminal moraines to the great Scandinavian ice-sheet are said to be forthcoming.

I have already quoted some examples of the rough-and-ready way in which Buckland and other early devotees of the glacial nightmare discriminated their moraines, and misled everybody accordingly. The same methods are not yet extinct. Thus Mr. Carvell Lewis, who ought to have known better, often speaks of "moraines made of stratified materials". On page 463 of his well-known book he describes such moraines as having "existed in valleys far from modern glaciers in low ground where water freely plays, or in an angle between two glaciers where water plays, *made purely by water*". The italics are his own. Moraines made purely by water are surely very curious ice-products, and I do not propose to contest their existence in Yorkshire.

The acute observer just quoted, who is a great authority among the ultra-glacialists, has examined this district with some care, and I will quote some of his conclusions. Of the neighbourhood of Driffield he says: "The drift is clearly aqueous and iceberg made. No two parallel sections, even if but a few yards apart, would show the same appearance in the arrangement of the beds. Large foreign boulders and

gravel also occur *standing on end*, or nearly so. A dove-tailing of clays is universal. The mounds of clay are not moraines, but have been dropped by icebergs" (*Glacial Geology of Great Britain*, p. 177). Of the country nearer the coast, he says, "at Gristhorpe are hummocks and ridges of boulder clay of rather morainic aspect"; but he adds, "the mounds are rounded as if either made in water or subsequently submerged" (*ibid.*, p. 206). "A range of drift hummocks runs from Buckton to Speelton which are sandy and gravelly. Similar sandy hummocks occur farther down, in the middle of Holderness, where at a low elevation (less than fifty feet above the sea) shells and shell fragments occur in them *showing that if it is a moraine, it was made under water*" (*ibid.*, p. 209). Of the so-called moraine at Flamborough Head he says: "These moraine mounds have been opened for tumuli, so regular is their shape. . . . They are made of angular gravel, the sandy layers being more or less horizontal, as if made by marginal waters. The boulders are not water-worn, but are shaken and shuffled and avalanched and roughly used by water and ice so that the striæ are more or less worn off suddenly. At another part of Flamborough Head . . . the drift is rudely and horizontally stratified, as if by water fringing the ice-sheet. . . . From Barmston southwards through Holderness the moraine hills are sandy and contain marine shells. . . . Beacon Hill is a sharp hummock. . . . This hill of drift is finely exposed on the sea cliff, and is seen to be sandy, gravelly and stratified within, the boulder clay being on the top. . . . Alternations of coarse gravel and sand occur, and it is especially sandy towards the bottom." Speaking of purple boulder clay in the district just south of Bridlington Quay, Lewis says: "This clay looks water-made here, not glacial, being in great part stratified with chalky bands; the loess lies on it in great sweeps. . . . Good cross-bedding is seen in the loess and alternations of gravel, also fine ripple marks." In speaking of a laminated clay in the boulder clay here, Mr. Lamplugh says: "It looks as if deposited in open water of no great depth, with no ice excepting light floes". Lewis argues that one-half of Holderness is glaciated and the other is not, the two being separated by heaped-up terminal accumulations of drift running the whole length of the region in a crescentic curve. These he seems

to treat as moraines, though he speaks of one of the principal drift hills, *i.e.*, Kelsey Hill, as containing unmoved estuarine sands filled with unbroken shells. He elsewhere speaks of the Hessle clay in Kelsey Hill as "an aqueous clay, and as clearly not till, but a deposit made in water, below which is red Philadelphia gravel interlaminated with and undulated by sandy layers"; and he adds in italics, "*Kelsey Hill was formed in water*". Crache Hill, south of Garton Station, he calls an interesting sea-beach, and the sand of which it is made a true sea-sand, which shows successive waves or tides in streaks of coarse and fine, dipping seaward, and underlaid by coarser gravel, the whole often false-bedded, and containing rolled clay boulders. Speaking of a mound of drift near Oulston, Lewis says it is a rudely stratified mass of coarse gravel and boulders which is quite clayey, but has sandy streaks. He accordingly calls it a mass of re-sorted moraine material of the so-called Allerton Park moraine, which he says was the medial moraine between the Wensleydale glacier on the west and the Stainmoor glacier on the east. He says, further, there is a good exposure at Hopperton. "*The mass,*" he says, "*is here rudely stratified, and it is a water-washed moraine stuff. The clay is in large part washed out, and the stones have for the most part lost their striations.* It is much like the moraine at Oulston. There are," he adds, "low, flattened gravel hills at Goldsburgh, but these appear to be merely gravel streams washed south from the moraine by glacial floods. . . . The deposit of clayey drift at Harrogate is not heaped up like a moraine and may belong to the Fringe." Lewis speaks of a so-called moraine in Studley Park as being "*marginal kames draining the moraine inwards*". What this may mean I leave to cleverer people than I am to explain. Bingley, Lewis describes as built on a moraine, a ridge of drift crossing the valley like a great dam. "North of Bingley," he says, "are mounds of stratified drift, a frequent occurrence at the back of the terminal moraine." At the back it will be noted, and not in front of it. How this could be I do not know.

Surely it is time to get away from this contemporary playing at science to the more logical methods of the old masters. "Among the peculiarities," says Phillips, speaking of these Yorkshire mounds, "may be reckoned the irregular mounds

of gravel and sand, which denote the ancient action of sea-currents, for all Holderness was a sea-bed in 'the glacial' period." Of these mounds he proceeds to describe the famous one at Brandsbarton, which in Norway, he says, would be called an "äs" and in Ireland an "escar" (*Rivers, Mountains and Sea Coast of Yorkshire*, p. 124).

This completes my survey of the chief external and internal features of the drift deposits in so far as they throw light upon and are a test of their having been produced and laid down by ice or by water; and it seems to me that it is impossible, when the details are actually sifted and analysed, and a really inductive method is applied to them, to come to any other conclusion than that the glacialists have deluded themselves by relying on very superficial and flimsy tests and criteria, and by altogether ignoring the great mass of difficulties which their theory, when thus tested, involves, and that both the external and the internal features of the drift deposits are quite inconsistent with their being products of ice, as we know it in nature anywhere.

NOTE.—I mentioned in describing the äsar or eskers their absence from the Arctic regions. Chamberlin, speaking of Greenland, says: "No eskers or kames were seen in process of formation except in miniature type . . . nothing distinct or typical (of eskers) was seen . . . so also in regard to the kames". The fact seemed to greatly embarrass Chamberlin. Again, he says: "No drumlins were seen in process of formation, nor were any seen in the abandoned territory unless we force interpretation in some doubtful cases" (see "Recent Glacial Studies in Greenland," *Bull. Geol. Soc. America*, vol. vi., 215-216).

CHAPTER XVII.

THE ARCTIC REGIONS IN SO-CALLED GLACIAL TIMES.

These are
The palaces of Nature, whose vast walls
Have pinnacled in clouds their snowy scalps,
And throned Eternity in icy halls
Of cold sublimity. —BYRON.

IN my previous work on the glacial nightmare (*Glacial Nightmare*, pp. 510, 511), and also in an earlier part of this work (vol. i., p. 199), I have collected the evidence which goes to show that the traces of so-called glacial action in the northern hemisphere instead of being circumpolar, are, in fact, limited to one-half only of the hemisphere, namely, to that bounded roughly on the east by the White Sea and on the west by the river Mackenzie.

This does not, however, exhaust the matter, and I have further ventured to suggest (*Glacial Nightmare*, pp. 453-457) that while North-Eastern America on the one hand, and Scandinavia, Finland, North Russia, North Germany, Holland and Britain on the other, offer abundant evidence of the kind which is supposed to necessitate the hypothesis of a glacial period, there is no such evidence forthcoming from the other lands within the Arctic circle, and notably from Greenland, Spitzbergen and Iceland; and that it would appear, in fact, that these lands during the so-called glacial age, far from having been smothered in ice and snow, enjoyed much more temperate conditions than they do now.

I now propose to examine this important matter at somewhat greater length. Before turning to the supposed evidence of former extensive glaciation in high latitudes, I wish first to make some observations of an *a priori* character which I think pertinent. The initial question necessary to the solution of

this problem is one which has seldom or never been actually faced. Why is it that the regions round the two poles are so shrouded in *perpetual* inland snow and ice? The answer that would occur to most people doubtless is that these regions are more or less permanently ice-bound because of their high latitude and the consequently long winters which prevail there, and that their condition is due to meteorological causes operating directly upon them, such as the alternation of heavy winter snowfalls and succeeding cool summers. Hence the difficulty in many minds of understanding how in former times the two polar areas should have had a fairly temperate climate, as the geological evidence clearly shows was the case. I altogether contest this view. It seems to me that the summer heat falling upon the two polar areas is now, and must always have been, quite sufficient to dissipate the effects of the previous winter's cold but for one fact, and this fact is the dominating one in Arctic geology. The fact in question is the existence of great masses of high land in both of the polar zones. Croll, among the other fantastic and subjective conclusions which he was continually urging, argued that the polar lands are flat plains upon which rest enormous ice-sheets of great depth and bulk. No conclusion could be more hapless and contrary to the facts. The vast continent of Greenland is 1,200 miles long and 500 miles wide, and is, as all the evidence shows, a high plateau terminating in scarped cliffs all round, which cliffs are cleft by great ravines called fiords, through which the ice which collects on its broad back in a vast mound pours into the sea in the form of fleets of icebergs. The southern continent, so far as we know, is the same. Let us shortly turn to the actual evidence on the point. This, in regard to Greenland, I have given at considerable length in my *Glacial Nightmare* (pp. 668-673). I will now merely supplement what I then said.

Reclus describes the contour of Greenland very clearly. He says: "The detailed study of the west coast, which is free from ice for a considerable distance, makes it sufficiently evident that Greenland proper forms a continuous mass of land. The existence of coast ranges, whose crests are seen towering above the ice in regular lines, the homogeneous character of the rocks examined in various parts of the country,

the form of the inlets along the sea-board, the general disposition of mountains and plateaux, all impart to Greenland an aspect greatly resembling that of Scandinavia. In both regions the contours are the same, and they would present an analogous appearance were the western land disencumbered of its icy fetters. As in Norway, the coast line is fringed with ramifying peninsulas continued seawards by islets and little archipelagoes, and there are lands which with the advance and retreat of the glaciers may alternately be attached and separated from the mainland.

"Throughout their whole length the coastlands are mountainous and of forbidding aspect. Even the southernmost point at the extremity of an archipelago is a gloomy mountain, the Kangak Kyrdlek or Umanarsuak of the natives, to which the English seafarers have given the name of Cape Farewell, and which the Scandinavians call Statenhuk. North of this headland the west coast is dominated by long serrated ranges with crests 'sharp as shark's teeth'. The mean altitude of these crests scarcely exceeds 1,600 feet, but in the interior of the southern point the peaks attain an elevation of over 7,600 feet. The inhabited regions in Danish territory have summits exceeding 3,000, and in some places even 4,000 and 5,000 feet, but north of the polar circle the mountains are less elevated in the region of deep fiords stretching north of Disko Bay. Here the sea-board rises in gentle slopes towards the ice-fields of the interior. But the rugged island of Disko itself, the largest on the west coast, presents crests and domes rising above 3,300 feet. Still farther north the peninsula of Nursoak has summits of 6,000 feet, while the peaks of gneiss on the neighbouring mainland rise to heights of 6,500 feet and upwards. Beyond this point the coast range falls, although the gaze of mariners is here attracted by the excentric form of the 'Devil's Thumb,' a lofty eminence terminating in a sort of obelisk. According to Kane, the Arctic Highlands north of Melville Bay nowhere exceed 2,000 feet; on the east side of South Channel, Hayes ascended a peak 4,170 feet high, and Nares attributed a height of 6,000 feet to a summit in Washington Land, the peninsula skirting the east side of Kennedy Channel.

"The east side of Greenland, indented like the west with

fiords and fringed with islands, is the loftier and more precipitous of the two, and here rises the highest mountain hitherto discovered. In 1870 the German expedition under Koldewey penetrated into an unknown fiord, the mouth of which was masked by over a hundred icebergs. This long and winding inlet, which was named the Franz-Joseph Fiord, is dominated by steep escarpments from 6,000 to 7,000 feet high, and consists of horizontal layers interspersed with quartz, schists and limestone. Towards its western extremity in the interior of the continent the pyramidal mass named Mount Petermann rises, according to Payer, to an altitude of at least 11,000 feet. Other summits of like elevation probably occur elsewhere, for the explorers have already observed domes 10,000 feet high in the southern regions, where Greenland is much more contracted than in higher latitudes. The backbone or water-parting between the two slopes, placed by Nordenskiold near the west coast, is by Rink and most other authorities removed to the opposite side, presenting its more precipitous slope towards the Atlantic. Most of the uplands denuded by the melting snows or retreating glaciers consist of crystalline rocks, such as gneiss, granites and porphyries. . . . Near Godhavn in Disko Island a basalt escarpment rises nearly vertically to a height of 2,000 feet, and above is seen the bluish section of a glacier, overhanging the precipice."

Jensen mentions a solid rock, one of the *nunatakker*, where he found a solitary bird which was 4,400 feet above the level of the sea and about twenty-four miles in the interior of the ice-field.

Peary speaks of the district on the plateau near the end of his journey in North Greenland as the crest of a range of rock-strewn mountains parallel to the great glacier to the east, a region of utter barrenness. The surface was covered by small angular stones compressed and half-cemented together on which were strewn larger loose fragments singly, in piles and in long moraines.

At length when twenty-six miles from "Moraine Camp," says the same writer, "after climbing over a rugged slope, over rugged rocks and through drifts of heavy wet snow we reached the summit, and a few steps more and the rocky plateau on

which we stood dropped in a giant iron wall that would give the Inferno 3,800 feet to the level of the bay below us. We stood upon the north-east coast of Greenland, and looking far off over the surface of a mighty glacier on our right and through the broad wide mouth of the bay we saw stretching away to the horizon the great ice-fields of the Arctic Ocean. . . . Our observation point was a giant cliff, almost vertical, overlooking the bay. . . . We could see beyond the thousand red boulders in the foreground, and through a depression in the hills the middle course of the broad ice river glistening in the sun. Across the glacier bounding the fiord on the east rose a long line of precipitous cliffs, higher even than the one on which we stood, and projecting several miles out into the bay. They rose 4,000 or more feet in sheer height above the glacier, and terminated in a grim promontory sloping steeply to the water. On their huge shoulders these wild cliffs supported a great projecting tongue of the inland ice. Some fifteen miles north-east these cliffs ended in a bold cape, which I named Glacier Cape."

Later on he describes a boat journey in Inglefield Gulf, the largest of Greenland inlets. He started from *Red Cliff*, rounded Cape Cleveland, "a typical bastion headland," free of snow, and passed the shores of *Red Cliff* peninsula, and a succession of deltas between the glaciers. "At the back of these deltas and a low foreshore, which connects them, run a series of rather rolling summits down the ravines, between which protrude hanging glaciers, tongues of the Central Ice Cap." Peary speaks of the warmth of colouring of the shores, barren though they were, and gives figures of high ground, with steep and precipitous sides, quite free from snow. East of Karnat he speaks of a line of majestic sandstone cliffs rising sheer from the water. "These cliffs," he says, "were of striking boldness," and he speaks of "creeping along under the mighty ramparts, in one place a Titan watch-tower, in another a giant amphitheatre, here a niche there a bastion, and between them and over them grouped rows of pinnacles, and the Esquimaux call them the Statue Cliffs. Silver threads of cascades fall down the cliffs in places." He gives a figure of one of the Titan watch-towers. "There we saw several reindeer. The water in the bay is almost

as red as spilled blood, from the fine red sandstone silt brought down by the sub-glacial streams. . . . At the farthest angle at the head of the fiord was a huge moraine thrown up by a glacier, the edge of which appeared over its top. Beyond that was an isolated mountain of striking boldness and sharpness of outline some 2,000 feet high." Close by he saw many white whales puffing, and he speaks of a tiny valley under the shadow of a vertical-faced mountain. Flowers and grass were abundant there. After passing East Glacier there were a mass of warm red-brown cliffs, in which bastions, towers and ramparts were so strikingly like some mediæval strongholds that he called the rocks Castle Cliffs. Then came Hubbard Glacier, then gneissose precipices, and farther on was a gulf surrounded by glistening glaciers separated by wild and towering mountains. He speaks of the striking peaks Mounts Adams, Daly and Putnam. To the east was a striking precipitous island whose cliffs he scaled for 1,500 feet, and from the top saw the glacier face interrupted by several precipitous-walled, flat-topped, isolated mountains or *nunataks* as the natives call them, and above them the slope of the Great Ice. In this bay they found a school of narwhals. He speaks of "the orient cliffs of an island in the gulf as a mass of rich brown colour. Scattered over its summit were numerous great erratics. . . . Beyond the Leidy Glacier the mighty stream of the glacier flowed down between rugged *nunataks* from the heart of the Great Ice. The water was like ink." Peary met a small settlement of Eskimos on this island; they used bows and arrows. The place is on the confines of the great deer pastures of the district. He speaks of the immeasurable yawning crevasses on Froe Glacier valley, and gives a very good photograph of "the Giant of Atanekerdluk," a weathered pinnacle of a trap dyke.

In climbing up on to the Great Ice, Peary was much troubled by the deep crevasses he had to outflank, some 50 to 100 feet wide, which prove so clearly the broken ground on which the ice is planted. In going back he crossed the head of a fiord which he thus describes. "Dark brown and red cliffs looked down into a grand vertical-walled cañon, reaching up towards our camp, and everywhere, north-west, north and east, black

and dark red precipices, deep valleys, mountains capped with snow, shadowed domes of ice stretched away in a wild panorama. . . . Always as I advanced the mountains of the shore grew into view before me, and on the 1st of July a wide opening, bounded on either side by high vertical cliffs, showed up to the north-east over the summits immediately adjacent to the Inland Ice." He was then about 5,000 feet above sea-level. He went straight for the red-brown mountains, and speaks of "clambering upon the confused rocks of a moraine 4,000 feet above the sea". Peary then speaks of setting off to climb a summit some five miles from the edge of the ice.

Northumberland Island on Inglefield Inlet he describes as a part of the same dark granite formation that walls Robertson Bay "in towering grandeur," while Herbert Island is a part of the same crumbling disintegrating sandstone which reaches from Cape Cleveland to Bowdoin Bay in Murchison Sound.

Near Academy Bay he speaks of an island so like the Matterhorn in contour that he called it the Little Matterhorn, and says his objective was one of the rocky islands, half-buried on the face of the glacier, destined probably to become a *nunatak*.

Speaking of a mighty ice stream, the Heilpron Glacier, larger than the Jacobshavn, Tossukatek or great Kariak glaciers, he describes it as *having been deflected to the north-west by an archipelago of small islands*, so that practically its entire outflow is north of the islands, and between them and the Smithson Mountains.

"The term 'inland ice,'" says Peary, "suggests to the majority of persons erroneous ideas. The surface is not ice, but a compacted snow. Elevated as the entire interior is to a height of 4,000 to 9,000 feet above the sea-level, mountains of the coast visible to the sailor sixty to eighty miles disappear beneath the landward convexity of the ice-cap when the traveller has penetrated fifteen or twenty miles into the interior, and he may travel for days and weeks without a break in the continuity of the sharp steel-blue line of the horizon" (*Intr.*, p. lxvii.).

"The coast of Greenland," again says Peary, "is bold and mountainous, cut by numerous deep fiords and protected by an

advance guard of outlying rocky islands. Some of these fiords extend inland a distance of sixty to eighty miles. . . . All there is of land, as we understand the term in Greenland, is a ribbon five to twenty-five (and in one or two places nearly to eighty) miles in width, along the coast, made up of mountains and valleys and deep branching fiords . . . supporting like a Titan dome the great ice-cap, beneath which the interior of the country lies buried. . . . Here the accumulated snow precipitation of centuries, in a latitude and altitude where it is practically correct to say that it never rains and the snow does not melt even in the long summer day, has gradually filled all the valleys of the interior until it has levelled them even with the mountain summits, and still filling higher through the centuries has at last buried the highest of these mountain summits hundreds and even thousands of feet deep in snow.

"The interior of Greenland to-day is simply an elevated unbroken plateau of snow, lifted from 5,000 to 8,000 and even 10,000 feet above the level of the sea; a huge white glistening shield some 1,200 miles in length and 500 miles in width, resting on the supporting mountains. . . . On this frozen Sahara of inner Greenland occurs no form of life, animal or vegetable; no fragment of rock, no grain of sand is visible. The traveller across its frozen waste, travelling, as I have, week after week, sees outside of himself and his own party but three things in all the world, namely, the infinite expanse of the frozen plain, the infinite dome of the cold blue sky, and the cold white snow—nothing but these" (*Intr.*, pp. xxxii., xxxiii.).

This view is also pressed by two excellent geologists, Garwood and Gregory. In reply to those who argue that although it is now perfectly plain that Greenland is girdled round with high cliffs and mountains, yet will have it that inasmuch as no rocks project above the Central Ice when one has advanced more than a certain distance from the coast, therefore the interior is like that of a hollow cup or basin, they say:—

"In the case of Greenland a great upward movement of ice is often assumed, on the ground that ice formed in the interior has to climb over a marginal mountain-chain. Thus

Prof. Crosby ('Englacial Drift,' *Amer. Geol.*, xvii., p. 225) tells us that it is the general belief of geologists that if Greenland were divested of its ice-cap it would exhibit continental relief-elevated margins and a depressed interior. This view of the geographical structure of Greenland appears improbable. Geologically, Greenland is very similar to Spitzbergen. It very likely consists of a high plateau of sedimentary deposits supported on a great block of archæan rocks, which are exposed round the margins. The hypothesis that there is a great depression in Central Greenland is not one upon which it is safe to base an argument" (*Quart. Journ. Geol. Soc.*, xlvii., p. 219).

This will more than suffice to show how completely fantastic Croll's idea was as a description of what Greenland really is. It is in part a high plateau of continental dimensions covered with snow. Nor is it the only high land in the Arctic circle.

"Spitzbergen derives its name from the pointed peaks seen while coasting its western side. The mountains there rise steeply from the beach to a very considerable height. Round Smeerenberg (Oily Hill) Harbour many of them exceed 2,000 feet in height. The Devil's Thumb on Charles Island is calculated by Dr. Scoresby to rise 1,500 or 2,000 feet, and Horn Mount, in the harbour of the same name, he states to be 4,400 feet high. The mountains on the west coast are very steep, many of them inaccessible, and most of them dangerous to climb. . . . The views of the coast given by Captain Phipps show dark craggy rocks, projecting everywhere in summer above the snow, and the Devil's Thumb, a crooked peak, is alike destitute of snow and verdure, but the high rocks are black with lichens" (Richardson, *The Polar Regions*, p. 205).

Again, we read of the same archipelago: "High mountains, reaching 4,560 feet in the Horn Sound Tind, cover its southern parts; while a wide plateau, with an altitude of from 1,500 to 2,000 feet and covered by a thick ice-sheet, occupies the north. . . . A long narrow island, Prince Charles Foreland, with peaks of nearly 5,000 feet high, runs parallel to part of the west coast of West Spitzbergen, from which it is separated by a narrow strait. A few peaks, estimated as

from 1,600 to 2,000 feet high, protrude above the snow and ice by which Stans Foreland and Barents Land are covered. . . . The island of North-East Land appears like a broad plateau covered by an ice-sheet 2,000 or 3,000 feet in thickness from which a few peaks protrude. . . . Many of the smaller islands in the Spitzbergen archipelago rise to a height of 1,500 or 1,700 feet" (*Enc. Brit.*, xxii., pp. 407, 408).

Koettlitz says of Franz-Joseph Land: "Although the land is so largely hidden by ice which often terminates in high faces at the sea-level, I do not think there is so great a thickness covering the land as might be supposed, for not only are ridges and dimples to be seen everywhere upon the ice-slopes, but the rock itself frequently protrudes along their ridges" (*Quart. Journ. Geol. Soc.*, liv., p. 641). "Though no rock has been seen to protrude at the actual summit of Cape Flora, yet on the north side at about 100 feet from the top on a line with the highest part of the cliff points of rock are seen all round a cirque-like hollow. The slope below these rocks becomes steeper, and is interrupted by a crevasse such as is always found below such protruding rocks. Small cracks intersect the slope lower down, which gradually assumes a lower angle. The cracks and crevasses cross the ice-slope in a direction parallel to the line of rocks, thus taking the semicircular form of the cirque in which they lie. . . . Raised beaches were seen in water-courses that had cut their way through the ice down to the rock below. . . . Ridges and dimples in the ice indicate the position of submerged rocks" (*ibid.*, pp. 641, 642).

Fielden, speaking of Novaya Zembla, describes its interior range as rising in a series of sharply peaked and serrated mountains, averaging some 2,000 feet in height. "At the time of our visit," he adds, "in the latter part of July, the land was generally clear of snow, little even remaining on the central ridges except in their higher valleys and gorges" (*ibid.*, lii., p. 731).

It is plain from all these facts that the larger land areas of high latitudes in our hemisphere are in many cases at high elevations, and that in at least one instance these high lands have continental dimensions, namely, in that of Greenland. It is to this elevation and not to their high latitude that

is to be attributed the great cold of the North and South Polar regions. We all know that as we rise in the air the temperature falls, that there is a limiting line of elevation in all latitudes even in the tropics where the snow is perpetual, and that this line sinks as we get into higher and higher latitudes. It is very plain, therefore, what an enormous effect upon the temperature of these high latitudes must be produced by the existence there of a vast continental area at an elevation of several thousand feet above the sea-level. The ice-sheets of Greenland, of Spitzbergen and Franz-Joseph Land and the Southern Continent seem traceable to no other cause than this great elevation.

Its effect is not only direct but indirect. Directly Greenland, for instance, became covered with perennial ice and snow, this acted as a potent refrigerator in enormously reducing the temperature of the whole of the circumpolar lands. It acted first upon their own climate by preventing the summer heat from effectually warming the country, since it was engaged all the year round in melting ice and snow, and had no rocks or bare lands upon which to operate. It acted, secondly, in causing very cold winds to radiate from the ice-sheets in all directions and congealing with their frozen breath the waters of the shallow polar seas, thus helping the snows and ice on favoured land surfaces to resist the operations of the sun. It also scattered great loads of icebergs into the neighbouring seas and gave them their glacial temperatures. Peary describes the climate of Greenland at the present day with great force, thus:—

"There is no doubt in my mind that in the middle of the Arctic night, in the centre of this 'Great Ice,' lifted a mile and a half or two miles into the frozen air that sweeps round the pole, separated from any possible effect from the earth's radiated heat by a blanket of ice and snow a mile or more in thickness, and distant fully 250 miles from the possible ameliorating effects of the Arctic Seas, there is to be found the fiercest degree of cold of any spot upon the surface of the globe" (*Intr.*, pp. lxix., lxx.).

"At present," says Reclus, "the climate of Greenland is one of the coldest in the world. The isothermal of zero traverses the land near its southern extremity, and in the northern

districts whole years pass without a single summer's day, that is with a temperature of 59° or 60° Fahr. At Upernivik the glass falls in winter to − 47° Fahr., and even in summer it does not always rise to freezing-point. In September, Nansen and his party had to endure cold of − 56° Fahr. for several consecutive nights. On the other hand, the greatest summer heat rarely exceeds 64° Fahr. in the shade" (Reclus, *Geography*, xv., p. 79).

The way in which this intense cold affects the winds is well told by Peary. "The wind," he says, "is never quiescent on the 'Great Ice'. Day and night, summer and winter, year in and year out, it is sweeping down sometimes with greater, sometimes with less velocity from the frozen heart of the 'Great Ice,' bearing with it a burden of snow and following the most direct slope to the land, *i.e.*, the bare land, which once reached it goes rushing over the mountain summits, some of it sinking in whirlpools and eddies into the valleys, but much of it being carried on to the coast cliffs, over which it goes swirling into the sea or over the sea ice. . . .

"The regularity of the winds of the 'Great Ice' of Greenland, as I have found from a seven months' residence upon the 'Great Ice,' and visits to it of greater or less duration, at every month of the year, is phenomenal. Except during atmospheric disturbances causing storms, the direction of the wind is invariably radial from the centre outward perpendicular to the nearest part of the coast land ribbon. . . . The direction of the nearest land is always easily determinable in this way; the neighbourhood of great fiords is always indicated by a change in the wind's direction, and the crossing of a divide by an area of calm or variable winds followed by winds in the opposite direction, independent of any indications of the barometer" (*Intr.*, lxxiv., lxxv.).

So much for the winds, but quite as effective as porters of extreme cold and reducers of the mean temperature of the high latitudes are the enormous masses of ice detached from the great ice-sheets of the Arctic and Antarctic Oceans as icebergs every year, or formed by the breaking up of ice-fields "having an extent of several hundreds of thousands of square miles and constituting by their dimensions real continents". In the Arctic region and in spring time and summer these latter get partially broken up, and are carried into the open seas to the

south in enormous fields of ice, sometimes from fifty to a hundred miles in each direction. Many of the glaciers themselves are of enormous size. One of them seen on the banks of Newfoundland by the *Acadia* was about 2,180 feet high above the water and surmounted by a dome like that of St. Paul's, and its total height must have been over 3,500 feet. "A great number of these travelling masses have been seen measuring several miles in length and breadth whose bulk amounted to tens of thousands of cubic yards. . . . The enormous masses of icebergs like gigantic ships are often stranded in shoals even where the depth of the sea exceeds a hundred fathoms. . . . From the beginning of March to July and even August that part of the Atlantic east of the Bank of Newfoundland assumes the appearance of the Arctic Sea. The polar current descending from Baffin Bay parallel to the coasts of Labrador brings with it in long procession the fragments of the ice-fields and glaciers of Greenland. . . . It is principally in this region of the ocean that flotillas of ice are to be dreaded by navigators" (Reclus, *The Ocean*, ch. v., *passim*). The effect of this interminable procession of ice monsters upon the temperature of the North Atlantic must be enormous.

It seems plain, as I have said, that the really efficient cause of the terrible climate of the polar regions and of its secondary effect on neighbouring regions as here described is not their high latitudes but the high elevation of the land surfaces there, and if we could lower the level of these land surfaces very materially we should very materially raise their mean temperature and ameliorate their climate; and it is quite possible to realise that if the greater part of these lands now at a high elevation were reduced to near the sea-level we might have a fairly temperate climate even in the highest latitudes.

We need not rely merely on conjectures in regard to this fact. I have given some evidence about it in my *Glacial Nightmare* (see pp. 508, 509), which I now propose to supplement. Nothing in Peary's remarkable journey perhaps was such a revelation to some of us as his discovery that when he had reached the extreme north of Greenland and looked out towards the pole with a continuous ice-sheet of 1,200 miles behind him he found the low islands to the north free from

ice or snow, and found abundant animal and vegetable life in the ribbon of coast lands bordering the great plateau on the north and which are at a low level.

Speaking of the north of Greenland, he says: "The almost entire absence of snow on the northern country was a surprise as well as an annoyance to me, as it threatened seriously to interfere with the portage of the big sledge" (*op. cit.*, p. 470).

In the extreme north he killed several musk-oxen, hares and ptarmigan. He elsewhere describes the scene from the northern verge of the great icy continent where he stood. "There is every reason to believe that to the north-west., north and north-east, we were gazing upon an archipelago, whose western limits Lockwood had discovered in 1882. . . . The most distant land we could make out, far to the north-east looking over the point of Glacier Cape, must have been sixty miles away. It seemed to be flat topped, and there was *no ice-cap on it. . . . We looked in vain for any signs of ice-cap on the lands west and north-west of our point of view.* . . . From our point of vantage, three-eighths of a mile above the bay ice, the sound of a cataract came up to us from far below, and I was surprised to hear the familiar drone of a bumble-bee. We soon caught sight of the insect, which lingered near us for some time. The flies that buzzed around us were altogether too numerous to count. The day was delightfully warm and calm. The point was 81·37′·5″ N. lat. and 34·5 W. long." Before starting on his return Peary tells us how he plucked a handful of flowers from the rocks. "Bright yellow flowers were peering at us," he says, "from among the forbidding rocks. . . . During our traverse of the northern land," he adds, "I found flowers of numerous varieties blooming in abundance, conspicuous among them was the ever-present Arctic poppy." He also mentions seeing "snowbuntings, two or three sandpipers, a single Greenland falcon and a pair of ravens, two bumble-bees, several butterflies and innumerable flies. We saw about twenty musk-oxen along our route; the stomachs of the cows were filled with grass." He says they also killed a calf.

Peary is not alone in making this kind of observation. Speaking of North Greenland, Greely describes the country towards Newman Bay as "a level plain whose rocky, gravelly

surface *is but scantily covered with snow.* . . . This absence of snow as a covering for the ground," he adds, "not only in North Greenland, but in Grinnell Land, was general in our two years' experience, and caused much comment as contrary to expectations" (*Three Years, etc.*, i., p. 219). He speaks of an extensive plain stretching to the east as far as could be seen, where the absence of snow was surprising.

He thus describes the features of Grinnell Land. "There exists from Robison and Kennedy Channels westwards to Greely Fiord and the Polar Sea a series of fertile valleys clothed with vegetation of luxuriant growth, whereon pasture large herds of musk-oxen. Over a hundred of these interesting animals were killed, and over two hundred more were seen. The boundaries of this fertile region are equally extraordinary. To the north an ice-cap of several thousand square miles bursts through every gap in the Garfield and Conger mountains in the shape of large glaciers, one of which, Henrietta Nasmith, has a front of five miles and a perpendicular face from 150 to 200 feet high. . . . Yet the winter's scanty snow scarcely covers this favoured country, while its abrupt intersecting fiords and deep narrow valleys offer most favourable conditions for the action of the constant summer sun and the complete drainage of its rapid torrents" (Greely, *Scotch Geo. Soc.*, 1885).

Referring to the contrasted conditions of the flat district of Grinnell Land and the high icy plateau of Greenland, Fielden, a very experienced Arctic observer, has pointed out the "extraordinary difference now existing between the fertile belt of Grinnell Land and the opposite shore of Greenland covered by a *mer de glace*" (*Annual Address, Norfolk and Norwich Nat. Hist. Soc.*, 1886, p. 166). "While in Grinnell Land there are hardly any glaciers now descending to the sea north of 81°, on the same parallel in Hall Basin in Greenland the country is ice-clad to the water's edge" (De Rance and Fielden, *Nares' Travels*, ii., pp. 343, 344).

The same report (see *Glacial Nightmare*, pp. 507, 508) is given us by the various Arctic travellers of the summer appearance and comparatively rich flora and fauna of many other Arctic lands at a low level, such as the tundras of Siberia and the vast plains of Boreal America. If this is the

case now, what an effect would be produced if the great refrigerators in Greenland and Spitzbergen and Novaya Zembla were removed, and if the whole of the summer sun were employed in heating the Arctic lands instead of, as I have said, being so largely employed in melting a part of the snow-blanket there. We can hardly realise the strength of the sun-power in the two polar regions if the sun could only have his way, and not be thwarted by the vast permanent ice-houses above-named.

"In East Greenland the solar rays often appear unendurable to travellers. Payer relates that on the shores of the Franz-Joseph Fiord the sailors, overcome by the heat, fell into a lethargic sleep from which it was difficult to rouse them. Scoresby saw the natives on the east coast walking about naked to cool themselves" (Reclus, *op. cit.*, xv., pp. 79, 80).

Reclus says of this summer sun that it amply suffices to melt all the snow in the plains, and even on the hills of the coast lands of Greenland (*ibid.*, xv., p. 79). Nordenskiold tells us that in summer the Siberian tundra is completely free of snow (*Voyage of the Vega*, p. 378). Again, he says, "along the whole coast from the White Sea to Bering Strait no glacier was seen. During autumn the Siberian coast is nearly free from ice and snow" (*Nature*, Nov., 1879). In passing up the Yenisei in 1875 in August, he only saw snow in one place, a deep valley-cleft of some fathoms in extent.

If the above argument is a true induction, and if in fact the present climate of the two polar areas be due to the great elevation of certain of the Arctic lands in either hemisphere, it would seem plain if there was ever a great glacial period in these same latitudes, such as the glacialists have postulated, the land must have been considerably higher than it is now. This at all events is the only cause I can suggest (in the absence of all possible astronomical ones which have been shown to be inefficient) to produce such a result. Have we any evidence of such an enhanced elevation of the Arctic lands in the last period of the earth's history? On the contrary, all the evidence known to me converges on the conclusion that the great areas of land about the poles instead of being higher were formerly much lower.

It is many years ago since I argued, and, I think proved,

that the whole circumpolar area at either pole, wherever we can test it, is rising and has been rising from the sea-level for a long period. I will quote a few of the facts which I have published on the subject and add some others. In Franklin's voyage, in 1819, he tells us that he picked up some decayed wood far out of the reach of the water, at the mouth of the Coppermine River, which river, he tells us, has no wood on its banks, and does not now carry it (*Narrative*, p. 357). In his second voyage along the Arctic Sea he describes the coast from the Mackenzie River to the Rocky Mountains as very shallow and full of shoals and reefs. Inside some of the latter was brackish water, as was also the water in pools at some distance inland; piles of wood were also thrown up far from the coast (*ibid.*, p. 134). Dr. Richardson says: "On the coast from Cape Lyon to Point Keats there is a line of large drift-timber, evidently thrown up by the waves, about twelve feet in perpendicular height above the ordinary spring tides". He shortly afterwards mentions that in the Polar Sea, when cumbered with ice, such waves are impossible, and as his journey was in the hottest season, and as the sea was then crowded with hummocks, the inference that the drift-wood was thrown up by the waves is inadmissible, and the line of drift-wood twelve feet above sea-level is a clear proof of recent elevation. Richardson suggests that the vast sheet of shallow and brackish water 140 miles long and 150 broad, separated from the Polar Sea by low banks and spits of sand and called Esquimaux Lake, was only recently a bay of the Polar Sea, and is an example of the formation of huge brackish lakes by a sea which is constantly contracting.

The Arctic archipelago north of North America has all the appearance of an area only recently risen from an ice-laden sea. The rounded and polished surfaces of the islands, etc., etc., and the existence of whales' bones and drift-wood at considerable elevations, prove this amply. Thus, Mecham, who was one of Belcher's officers, found tree-trunks on the surface and in the soil of Prince Patrick Island. Belcher found on the Victoria archipelago and in Grinnell Land bones of whales and other sea animals at a height of 280 metres above the sea.

"McClure found shells of *Cyprina islandica* at the summit

of the Coxcomb range, in Baring Island, at an elevation of 800 feet above sea-level. Captain Parry has also recorded occurrences of *Venus* (probably *Cyprina islandica*) in Byam Martin Island, and in the later voyage of the *Fox* the surgeon found the following sub-fossil shells at Port Kennedy, at elevations of 100 to 500 feet: *Saxicava rugosa, Tellina proxima, Astarte arctica* (*borealis*), *Mya Uddevallensis, Mya truncata, Cardium sp., Buccinum undatum, Acmea testudinalis,* and *Balanus Uddevallensis*" (Appendix to MacClintock's *Narrative*). The bone of a whale was found by Mr. Walker at a height of 164 feet.

Speaking of the eastern part of Melville Island, Parry says: "Sergeant Martin and Captain Sabine's servant brought down to the beach several pieces of fir-tree which they found nearly buried in the sand at the distance of 300 or 400 yards from the present high-water mark, and not less than thirty feet above the sea-level". Again, speaking of the northern part of Melville Island, he says: "Near Point Nias two pieces of drift-wood were also found on the beach, ten or twenty feet above the present level of the sea, both of pine, one seven and a half feet long and three inches in diameter, and the other much smaller. Both were partly buried in sand, and their fibres so decayed as to fall to pieces on being laid hold of." Again, speaking of the west of Melville Island, he says: "The land gains upon the sea, as it is called, in process of time, as it has certainly done here from the situation in which we found the drift-wood and the skeletons of whales" (Parry, *Voyage in* 1819-20, pp. 68, 193, 235).

MacClintock says of the same island that it is rather low, the western shore extremely so, and bears evidence of a gradual and tolerably recent upheaval from beneath the sea (*Quart. Journ. Geol. Soc.*, xxx., p. 10).

It is well known that Greenland is at present subject to a movement of oscillation, the northern portion being in process of elevation and the southern of depression; the axis of the movement being variously placed between the parallels of 74 and 77. Dr. Kane says: "The opportunity I had today of comparing the terrace and boulder lines of Mary River and Charlotte Wood Fiord enables me to assert positively the interesting fact of a secular elevation of the crust commenc-

ing at some as yet undetermined point north of 76° N. lat., and continuing to the great glacier and the high northern latitudes of Grinnell Land. This elevation is connected with the equally well-sustained depression of the Greenland coast south of Kingutak" (Kane, *Travels*, ii., p. 80).

The evidence for this elevation is especially marked in the neighbourhood of Smith Sound, Kennedy Channel, Robeson Channel, etc., etc.

Kane noticed some high beaches in Wolstenholme Sound. He found others at Mary Minturn River, 78° 45′ north of Van Renslaerhaven. He here counted as many as forty-one of these raised beaches, showing as many successive rises of the land.

Hayes says: "While rounding the head of Gould Bay, I observed that, as at Port Foulke, Van Renslaer Harbour, and, indeed, in almost every bay of the Greenland coast which I have visited above Cape York, the land rises with a gentle slope, broken into steppes of greater or less regularity, a series of terraced beaches, the highest of which I estimated to be from 120 to 150 feet above the sea. . . . They indicate a consecutive elevation of the two coasts" (*Open Polar Sea,* 1867, p. 303).

Speaking of Port Foulke, Hayes also says: "The terraces there are twenty-three in number, and rise very regularly to an altitude of 110 feet above the mean tide-level. The lowest rises 32 feet higher than the tide, but above this they climb with great regularity. They are composed of small pebbles rounded by water. . . . They occur in all similar localities, and illustrate the gradual upheaval of that part of Greenland lying north of latitude 76°. . . . I have before mentioned the evidences of a similar elevation of the opposite coast found in the terraced beaches of Grinnell Land" (*ibid.,* pp. 362, 363).

In Carl Ritter Bay, 80° 45′, and other places, Hall found numerous marine remains on the tops of the hills as well as on lower ground as far north as 82°. In Nares' expedition drift-wood was found west of Robeson Channel at a height of 47 feet above the sea.

Some interesting evidence of another kind of the rise of Northern Greenland is given in the geological appendix to

Nares' account of his voyage. Thus we read: "The molluscan fauna in the glacio-marine deposits of Grinnell Land and North Greenland at various levels up to 1,000 feet above sea-level is practically identical with that now living in the neighbouring seas, and the species *Pecten grœnlandicus*, *Mya truncata* and *Saxicava rugosa* are alike most abundant in the modern seas and in the older mud beds; and it is exceedingly worthy of note, as indicating the comparatively modern elevation of this coast line, that stems of two species of *Laminaria*, which grow in considerable abundance in the Polar Sea, occur in the mud beds at elevations of 200 feet, still retaining their peculiar sea-shore odour.

"Coniferous wood, still retaining its buoyancy, occurs at elevations of several hundred feet of a precisely similar character to that now stranded on the existing coast line. No evidence was discovered in the mud beds of Grinnell Land to encourage the idea that any of these trees had grown *in situ*, or that during the period occupied by the elevation of this tract of country 1,000 feet, it had experienced an interglacial period during which such trees could have flourished" (De Rance and Fielden, *Nares' Narrative*, ii., pp. 342, 343).

Some of the Greenland lakes were evidently once marine inlets; such is one discovered by Kane north of the Humboldt Glacier, some thirty miles higher than spring tides. Its water has gradually become fresh, but its fauna remains marine. Round about Polaris Bay, Hall visited several basins of a similar character, and up to an altitude of 1,760 feet he observed beaches containing thick beds of drift-wood and marine crustaceans (*op. cit.*, pp. 75, 76).

At Akpani on Wolstenholme Sound, Peary describes a triangular bit of foreshore made of detritus, a raised beach, in fact, formed originally under the water by tidal eddies and then raised to its present position by the elevation of this region (Peary, ii., p. 333).

In East Greenland, Payer found on the north coast of Shannon Island, on the south of Sabine Island and between Cape Broer Ruys and the Mackenzie Fiord raised beaches at a height of 100 metres, and describes them as covered with a fine marine loam.

The south of Greenland is now apparently slowly sinking.

Of this many evidences have been quoted, chiefly drawn from the old sites of Esquimaux huts, etc., which are now washed by the sea. This sinking seems, however, to be a quite recent movement, for Brown mentions raised beaches as occurring in the south as well as the north of Greenland, and these very huts are planted on what were once raised beaches containing shells which are now sinking again.

In regard to Spitzbergen the fact of its recent rise was long ago recognised. Thus we read as early as 1646: "Leonin was not a little surprised to discover upon one of these hills, about a league from the seaside, a small mast of a ship with one of its pulleys still fastened to it. This made him ask the seamen how that mast came there, who told him they were not able to tell, but were sure they had seen it as long as they had used the coast, and the writer suggests that the sea was once there" (Pereyre, *Churchill's Voyages*, ii.). Parry in his journey towards the Pole (*op. cit.*, p. 126) also refers to the vast quantities of drift-wood stranded on the Spitzbergen coasts above high-water mark. Lamont says (*Quart. Journ. Geol. Soc.*, xviii.) that he discovered recent bones and drift-wood several miles inland in Spitzbergen, and high above high-water mark skeletons of whales 30 to 40 feet above the sea-level. The seal-fishers told him the land was rising, and that the seas thereabouts were now too shallow for the right whale, which had forsaken the Spitzbergen coast. This is confirmed by Malmgren (see *Petermann's Mittheilungen*, 1863, ii.).

Lamont reports that he found drift-wood from Siberia wherever he went in Spitzbergen as at Star Fiord or Wijde Jan's Water, Deevie Bay, Walter Thymens Road and the so-called Thousand Islands. This occurred as high as 9 metres above the sea-level. He also mentions finding on one of the Thousand Islands a *Furche* 91 metres long, which could only in his view have been left there when the sand was submerged and by an iceberg. Theodor V. Heuglin and Count Waldburg-Zeil on their voyage in 1870 found large quantities of drift-wood in sharply defined raised beaches on the east coast of Stans Foreland, while the muddy soil of the country between the mountains and the sea seemed clearly deposited by the sea. The old Dutch maps of Northern Spitzbergen show islands now joined to the mainland. Dr.

Hahn gives some examples of these in his monograph, *Unters. ueber das Aussteigen und Sinken der Küsten* (pp. 127, 128). Hahn judges from the height at which certain deposits of drift-wood mixed with the débris of whale-fishers' handiwork are found at a height of 3 to 4½ metres that they must date from after 1650 that the land about Hinlopen Sound has risen about 2 metres per century, while farther east in King Karls Land the rise has been greater, since there similar deposits are found at a height of 6 metres (*ibid.*, p. 129). Nordenskiold tells us how Robert examined some layers of earth containing shells of the genera *Mya, Tellina* and *Saxicava.* Lamont discovered at Bel Sound, 100 feet above the sea-level, and at a distance of half an English mile from the shore, a decayed vertebra of a whale, as well as the lower jaw of the same animal at Walter Thymens Strait, at a height of 40 feet above the level of the water. "We, too, along the whole coast of Spitzbergen, except *the north-western part*, have discovered plain indications of an upheaval of land progressing in later times. Thus at Cape Lovén on the north coast of North East land we discovered along the shore a sand-bank in which, at the height of 10 or 15 feet above the present level of the sea, were found some hafts of whale harpoons, oars of a form different from that at present employed by the Norwegian sea-horse hunters, and other remains from the time of the Dutch whalers. Bones of whales and immense masses of moss-covered drift-wood were met with 1 to 20 feet above the level of the sea, on the isthmus that separates the mountain tops of the Seven Islands, far in the interior of Low Island, at the Ryss Islands in Murchison Bay, and at many other places. Sub-fossil shells were collected at a great height above the surface of the sea on the Lime shore and at Duyne Point in Hinlopen Strait. On the west side of Safe Haven (at least 150 feet above the level of the sea) Nordenskiold points out that many of the bones of whales found as above described were not stranded, but belonged to animals killed by the whalers in the sixteenth and seventeenth centuries, whose bones were originally embedded in the sand 3 or 4 feet beneath the surface of the sea, which sands have since been upheaved" (*Sketch of the Geology of Spitzbergen*, pp. 10-12).

Speaking of Northbrook Island in Franz-Joseph Land, Nansen says: "It presents evidence of changes in the level of the sea. Jackson's hut lay on an old strand line or terrace about from 40 to 50 feet high, but there were several other strand lines, both lower and higher. Thus I found that Leigh Smith, who had also wintered on this headland, had built his hut on an old strand line 17 feet above the sea-level, while at other places I found strand lines at a height of 80 feet. . . . Jackson had found whale skeletons and a skull of a balæna, at a height of 50 feet, near his hut. Farther north were fragments of a whole skeleton, but these were not more than 9 feet above sea-level. I also found other indications that the sea must at a comparatively recent period have risen above these strand terraces. For instance, they were at many places strewn with shells" (*Farthest North*, ii., pp. 481, 482).

Speaking of the same country, Dr. R. Koettlitz says: "Raised beaches of various heights occur at Cape Flora. Thus Elmwood is situated on a well-marked beach which slopes gently from 50 to 36 feet. This beach is prolonged into an apron-like front, having an area of about three or four acres. The surface is strewn with large and small water-worn boulders. . . . Many other beaches besides that on which Elmwood stands may be recognised at Cape Flora. At the north-west point of the cape well-marked terraces occur at 30, 35 and 80 feet above sea-level, and between this point and Elmwood others may be observed at 8, 29, 45, 54 and 65 feet" (*Quart. Journ. Geol. Soc.*, liv., p. 622). "On Mabel Island . . . in one place on the south-east side a well-marked beach was seen at a height of 300 feet, and traces of another at 410 feet above sea-level. . . . Bell Island is mostly composed of terraces of raised beach covered with rounded stones and pebbles of basalt and quartz, etc. . . . Around a bell-shaped mass of basalt 938 feet high may be seen a fine series of raised beach terraces up to 300 or 400 feet above sea-level" (*ibid.*, pp. 630, 631). "At Cape Forbes the usual talus and raised beaches occur at 25, 50 and 80 foot levels. . . . At Cape Stephen the 50-foot beach is well marked, and I found a portion of a reindeer's antler sticking out of it" (*ibid.*, p. 631).

"Two more specimens of reindeer antlers were found by

Mr. Bruce and myself, one at Windy Gully, the other on 50-foot beach, Cape Flora; and Mr. Leigh Smith mentions the finding of a fourth. At Cosoks Rocks there is a well-marked beach, 50 feet above sea-level, on which was a large trunk of drift-wood, about 20 feet long and 2½ feet in circumference, in very good condition. At Cape Grant is a partially concealed raised beach 120 feet high. On the plateau at the top of the cliffs at Cape Neale, 700 feet high, were found the entire skeleton of a seal and numerous bones of foxes" (*ibid.*, p. 632). "West of Cape Mary Harmsworth is a succession of beach terraces. I counted as many as twelve, and upon most of them drift-wood, birch bark, bones of whales, seals and bears, and big blocks of red and grey granite and gneiss were to be seen" (*ibid.*, p. 632).

Speaking generally, Koettlitz says: "The frequent occurrence of raised beaches around the shores of Franz-Joseph Land was alluded to by Messrs. Newton and Hall. These evidences of upheaval of the country in comparatively modern times are a marked feature of the whole of the southern part of the archipelago, and are known to occur in the northern parts also. Most of the raised beaches are between the sea-level and 80 or 100 feet above it; but in some places they may be traced as terraces to as much as over 400 feet above the sea. Indeed, many round pebbles which were found on the summits of Cape Forbes, Cape Flora and Cape Gertrude seem to me an indication of raised beaches even at these elevations, but being more ancient than those nearer the sea-level, much of their character has necessarily been destroyed by denudation. Drift-wood and bones of whales are not uncommon at the lower levels, and have been met with as much as 90 or 100 feet above the sea. The bones of seals and walrus have been found at much greater elevations (between 300 and 400 feet), and although they may have been carried up by bears, I hardly think so, for I know of no evidence that bears carry their prey to a distance, especially the whole of a seal (a complete skeleton was found on the summit of Cape Neale).

"Even when the land is covered with ice, it not unfrequently happens that ridges and sometimes successive terraces may be seen protruding. On these have been found many rounded

water-worn stones, some indeed being rounded boulders of enormous size, mixed with angular moraine débris. . . . Two or more well-marked raised beaches may be seen round Cape Flora at between 8 and 100 feet above sea-level, and the epiphyses of a large whale's vertebræ were found on the highest of them. Other terraces with rounded water-worn stones and pebbles, as well as walrus and seal bones, are less distinctly visible between 240 and 340 feet above the sea.

"Cape Gertrude has similar raised beaches at different heights. On the western side, at an elevation of 300 feet, large and small rounded water-worn stones and also seals' bones were found with much angular débris. On the eastern side of the valley, separating Cape Gertrude from the high land of Northbrook Island, are several terraces between 180 and 220 feet above the sea. . . . Farther east is one at an elevation of 220 feet. This is strewn with gigantic basaltic boulders many of them water-worn, being smooth and rounded but having no striæ. One of these angular boulders, a column of basalt, was 18 feet long and 6 feet in diameter."

Mabel Island has on the eastern side an area several acres in extent, 300 feet above the sea, paved with smoothly rounded water-worn stones. This Dr. Koettlitz says is a raised beach.

On Bell Island are very extensive raised beaches covering several square miles, generally 20 to 30 feet above the sea, but a few are as much as 300 feet. They are covered with well-rounded stones and pebbles. At many other places similar evidence was found, *inter alia*, at capes Grant, Neale, Crowther, Stephen, Forbes and Mary Harmsworth, as well as Windward Island. Water-worn stones are found on the summits of capes Neale, Grant, Forbes, Flora and Gertrude (*ibid.*, pp. 638-640).

The coasts of Iceland are everywhere rising. On the north coast banks of recent shells and of drift-wood occur to a height of 60 metres above the sea-level, and according to Olafsson and Pälsson this movement is a rapid one. In the north-west of the island, on the banks of the Breidi Fiord, the inhabitants point out numerous inlets, rocks, etc., which have risen from the sea during the last century (Reclus, *op. cit.*, iv., p. 922), while Van Hoff tells us that in 1753 a rock appeared above the water, in Flate Fiord, which had not before been

seen (see Hahn, *op. cit.*, p. 124). Evidences of a similar kind have been quoted from Jan Mayen, and notably from a comparison of the older maps of Scoresby and Carl Vogt with the later one of H. Mohn (1877), showing, apparently, the gradual conversion of islands into peninsulas and lagunes into dry land. The Norwegians in 1877 found drift-wood on the inner shores of lagunes there which no longer have access to the sea (see Hahn, *ibid.*, p. 125).

Turning eastward we come to Novaya Zembla. As long ago as 1664 we find Captain Wm. de Vlaimigh, who sailed along its north-east coast, saying that at a considerable height on a rock on the smallest of the three islands of Olange he found a very long tree that three or four men could not lift. This tree was rotten, and lay much too high to have been brought there by water (*Proc. Geol. Soc.*, ix., p. 168).

Fielden having very emphatically denied the former existence of any ice-sheet in Novaya Zembla, goes on to speak of all the islands he was able to visit there as having deposits of boulder clay lying on their undulations and hollows. He says he met with sections 20 feet in depth. The clay is of the same colour as the rocks on which it rests, and the included stones are angular fragments of the same rock. "I did not," he says, "detect an erratic or a rounded stone or an ice-scratched stone in any of this boulder clay. In many places it is full of shells of marine mollusca, *Saxicava arctica* predominating, though I found other species common enough. In some localities one might gather these shells by the bushel, few of them broken, never triturated, and in some cases the two valves are in contact. This description also holds good of the part of Greenland I visited, the abraded ridges, the deposits of boulder clay in the troughs and the presence of shells of mollusca all being very characteristic features." He accounts for these widespread deposits of boulder clay with the shells in them as due to the action of floating ice (*Quart. Journ. Geol. Soc.*, lii., pp. 731, 732), and urges that they point in fact to the recent elevation of the islands above the sea.

"The fact remains," says Fielden, "that beyond all doubt Novaya Zembla was submerged beneath the glacial ocean at an epoch contemporaneous with submergence by its waters of vast tracts of Europe, Asia and America. This submer-

gence caused a diminution of the glaciers of North Novaya Zembla and freed the southern parts from them. . . . Now the phenomenon is reversed: Novaya Zembla is rising, glaciers and *névés* are growing in size, and if the process continues to a certain stage, the whole of Novaya Zembla will be once more clothed with ice, and will present the appearance of an ice desert such as we now see north of Cross Bay" (*op. cit.*, pp. 266, 267).

At Gubina Bay, Novaya Zembla, Pearson speaks of a shingle beach showing several terraces, on the highest of which was a much decayed tree 25 feet long and 15 inches in diameter (*Beyond Petshora*, p. 129). Saline lakes have been found in Novaya Zembla which point precisely the same moral.

East of Novaya Zembla, Captain Mark, who made a journey there in 1871, found the barren and sandy islands known as the Gulf Stream Islands. In the spot where these now are the Dutch in 1594 found and measured a sand-bank in soundings of eighteen fathoms, showing an upheaval here of 100 feet in 300 years. In the same year Captain Nils Jonson landed in the country called Wiche Land in the map of 1617, situated about 30 E.L. and 78 N.L. He says that the shores there, for a distance of 100 miles inland and to a height of about 20 feet above high-water mark, were covered with drift-wood (*Ocean Highways*, pp. 247, 292). Turning to the island of Waigatz, which lies between Novaya Zembla and the mainland, and has been recently carefully explored, Pearson says:—

"There can be no question that the entire island of Waigatz has in very recent geological times emerged from the ocean. It has no mountains. Its highest ridges probably do not exceed 300 feet, whilst, as a rule, they are much lower; the troughs and valleys are very often filled with marine glacial clay of the same character as that now forming under water in the bays and around its shores. These deposits on the land contain molluscan remains and tests of foraminifera of the same species as we now dredge up from the neighbouring sea. At the present day no permanent snow or ice remains on Waigatz, nor are moraines to be met with, nor any signs of rock glaciation by land ice which might not plausibly be assigned to glacio-marine action.

"We meet, however, with rock glaciation on the summits

of ridges. . . . At lower altitudes we may observe the rounded ice-polished basement rock standing up from the marine-boreal clay of the tundra like rounded ice-worn skerries from the sea. . . . When we stand on the sea-shore, we find precisely the same polishing and rounding of the rocks and islets as we may observe on the ridges, only the polishing at the tide line and below the water is of a more general and apparently more recent character than that which we meet with on the higher ridges of the island. The polishing of the hard silurian limestone on which the beacon at Cape Greben stands is a good example of the rounding of much of the present coast line of Waigatz" (*op. cit.*, pp. 270, 271).

"Round Beluga Bay at the Matyushin Shar and on both sides of its eastern outlet we find raised beaches. Cape Vuchadnoi at its eastern outlet is a bold headland built up of four or five stupendous sea-beaches. The highest is about 500 feet above the sea, and they are separated from each other by slopes of about 100 feet in height, with broad and level terraces between them. A similar series of colossal terraces occurs on the opposite shore of the strait, and can be traced for many miles. The 500-foot terrace can be traced for miles around Beluga Bay. We found sections of it at this height where accumulations of shells existed, chiefly *Saxicava arctica* and *Astarte borealis*.

"The 100-foot terrace which winds round the hillsides of Beluga Bay is especially interesting, as it has a common origin with a series of outlines in the shape of rounded hills and eminences, now detached to some distance from the line of terrace, and bordering the present sea-shore. . . . Northward of Seal Harbour one of these eminences is exactly 100 feet high, is composed of rounded stones, sand and gravel, and formed of precisely the same materials as the terrace skirting the slopes of the hills at a quarter to half a mile distant. Several other similar hills are to be seen along the eastern shore line of Beluga Bay, between Seal Harbour and Freddy Straits. If their origin was not so patent we might have confounded them with eskers.

"At elevations up to 1,000 feet on the hillsides bordering the Matyushin Shar we came across patches of rounded water-worn pebbles that seemed to be remnants of still loftier sea-

beaches. . . . It is a very remarkable fact that the medial moraine of the Ibis Glacier in Zuvolka Fiord is largely composed of water-worn pebbles as round as golf balls. The water-worn pebbles undoubtedly came from *nunataks* some ten miles inland. It appears probable, says Fielden, that the glacier was eroding old sea deposits, and transporting their rounded pebbles over the glacier. These *nunataks* cannot be less than 1,000 feet above the present sea-level. . . . In other parts of the polar regions the changes of level are equally marked, for in Grinnell Land post-tertiary deposits are widely developed at altitudes of 1,000 feet" (Pearson, *op. cit.*, pp. 272, 273).

Fielden compares these terraces with the deltas now forming on the coasts of Novaya Zembla, which, he says, they exactly resemble. Sections of the terraces, he adds, some 100 feet thick, show them to be of a very heterogeneous structure of sand, gravel, rounded pebbles and rounded stones; they present no signs of stratification, although they are formed of mixed materials deposited on a large scale in shallow waters and exposed to the tides of a Novaya Zembla fiord (*op. cit.*, pp. 273, 274).

Describing the great deposits of sand and clay at Cape Matyushin, on the east coast of Waigatz, which extend for several miles, he says they fringe the coast as a range of low hills 50 to 100 feet in height. "The sections we examined show no definite signs of stratification. In this they agree with the beds at Kolguef, likewise in the number of ice-scratched boulders which both contain. Some of these at Matyushin are of great size; one of *biotite* granite was almost a perfect sphere as high as a man. Mr. Joseph Wright detected foraminifera and sponge spicules, the latter in abundance in these beds." Fielden argues from the want of stratification and the large boulders scattered throughout the mass that they were deposited in a sea deep enough to be removed from the influence of tides. These Matyushin beds pass by almost imperceptible gradations into the grey marine clays, with shells of recent mollusca that now form the surface of the present tundra land. "The grey foraminiferal clay is the most widely dispersed deposit. We find it spread over the tundra land of Arctic Russia, over Waigatz and over

Novaya Zembla, where it is to be met with in valleys and the slopes of hills up to an altitude of at least 500 feet, always forming the surface layer. Throughout its distribution it is very homogeneous in character. Precisely the same clay comes up in the dredge or on the flukes of the anchor in the Straits of Yugor, in Dolga Bay, and other anchorages on the coasts of Waigatz. When brought up from the bottom of the sea it is an unctuous mud; in the land deposits it turns into a stiff tenacious boulder clay. It is in this deposit that shells of recent molluscs frequently occur, and every sample that has been examined by Mr. Wright shows that it is rich in the tests of foraminifera" (*ibid.*, pp. 274, 275).

Mr. Wright in his report says that foraminifera occurred in all the samples of clay submitted to him by Fielden, all apparently of living forms. Two species of *Cassidulina* were much larger than living specimens, many specimens being three diameters larger, or twenty-seven times the size in bulk of these species as now found in the British Isles. "Nearly all the stones which occurred in the clays were more or less rounded, and presented the appearance of having been worn by marine action. Wright suggests that when the foraminifera were frequent the clays were deposited in still waters, and when infrequent, in rapid currents" (*ibid.*, p. 298).

In a ravine in Waigatz, Pearson says he saw the largest collection of drift-wood he had seen in the Arctic. All the wood was very old and decayed; some of the trees were 35 to 40 feet in length and over a foot in diameter. "The mass of drift-wood extended quite 300 yards up the valley, above the highest recent tide-marks; and that at the top must have been 35 feet above present sea-level, telling of a considerable elevation of the land since it floated in from the sea. From what I have observed of wood in the Arctic, I have little hesitation in saying that none of it had been there less than 300 years, and some probably much longer" (*op. cit.*, p. 111).

Pearson speaks of Kolguef Island, which is situated in the throat of the White Sea, "as a great bank which has been gradually raised above the sea-level during recent geological time, together with the whole northern coast of Europe" (*Beyond Petshora Eastward*, i., p. 20). Turning to the Continent, Murchison, Keyserling and Verneuil long ago described the

marine loam and sand containing shells of living species spread over the wide tundras 250 miles to the south of the White Sea and on the banks of the Dwina and the Vaga, the shells in which had not lost their colours. The Bremen expedition of 1876 brought home from the tundra deposits of the Lower Ob fifty-one kinds of sub-fossil shells like those still living in the Kara Sea.

M. de Middendorf states that the ground of the Siberian tundras is in a great part covered with a thin coating of sand and fine clay, exactly similar to that which is now deposited on the shores of the Frozen Ocean. In this clay, which contains in such large quantities the buried remains of mammoths, there are also found heaps of shells perfectly identical with those of the adjacent ocean. Far inland, besides, traces of drift-wood are seen, the trees which once grew in the forests of Southern Siberia; these trees, having been first carried into the sea by the current of the rivers, have been thrown up by the waves on the former coast, which are now deserted by the sea (Reclus, *The Earth*, ii., pp. 627, 628). Our chief authority for the shores of the Arctic Sea is Von Wrangel, and from his travels I shall quote freely. "In 1810, Hedenstrom went across the tundra direct to Utsjansk. He says that on the tundra, equally remote from the present line of trees, among the steep sandy banks of the lakes and rivers, are found large birch-trees, complete with bark, branches and roots. At first sight they appear well preserved, but on digging them up they are found to be in a thorough state of decay. On being lighted they glow, but never burst into flame; the inhabitants use them for fuel; they call them Adamoushina, or, of Adam's time. The first living birch-trees are not now found nearer than three degrees to the south, and then only as shrubs." Again, in 1811, Sannikof reports that he found the skulls and bones of various animals in the interior of Kotelnoi Island, and that both there and in New Siberia he found large trees partially fossilised. These islands have apparently all been recently submerged, for it is reported that the greatest stores of mammoth ivory are now got from the sand-banks which are constantly appearing near the Bear Islands; the barren surface of the latter, a conglomerate of bones, stones and ice, has all the character of a recently

recovered sea bottom. Wrangel tells us that ribs of whales are often found on the west coast, and that whales are now very seldom seen on the Siberian coast, while in the eighteenth century their appearance there was much more frequent. The cause for this desertion is that assigned by the Spitzbergen fishermen, namely, that the sea is becoming too shallow for the whale. "The shores of the Polar Sea, from the Lena to Bering Strait, are for the most part low and flat. In winter it is hard to say where land ends and sea begins. A few versts inland, however, a line of high ground *runs parallel with the present coast*, and formerly no doubt constituted the boundary of the ocean. This belief is strengthened by the quantity of drift-wood found on the upper level, and also by the shoals that run far out to sea, and will no doubt become dry land." Again: "At several places along the coast we found old weathered drift-wood at the height of two fathoms above the present level of the sea, while the fresh drift-wood lay on a lower level. *This indicates change of level.*" Again: "Captain Sarytschew says the winter dwellings erected by Laptef on the bank where his vessel was driven on shore lead to the belief that the channel must formerly have been on that side. At present there is no water there for a vessel of any size, and even a boat can only approach at high water. At low water the shoal runs three versts out to sea" (Von Wrangel, Sabine's Translation, cvii.). Diomed Island, described by Chalavrof in 1760, and by Laptef at a later date, no longer exists: it now forms a part of the main. The same voyagers describe the east coast of the Swatoi Noss as very sinuous: it is now very straight, the sinuosities having meanwhile disappeared. These facts will suffice to prove that so far as we have any evidence, the whole Siberian coast, as far as Bering Strait, is rising from the sea.

In Mr. Grieves' translation of the *History of Kamtchatka* (p. 54), I find it stated, in the description of Bering Island and the adjacent island, that thirty fathoms higher than the sea-mark lie wood and whole skeletons of sea animals which have been left by the sea. He speaks of one of the rivers at Ochotsk as being now dry; this is probably caused by upheaval. And in describing the Penschinska Sea, he says he had seen "trees which are not to be found in the country

hanging out of the earth, and more than seven feet below the surface"; whence, he says, it may be concluded that all these barren, boggy places, where at present there are no woods but shrubs and stunted sallows and birches, were once covered with water, which has decreased here, as it has on the north-east coast (Von Wrangel, Sabine's Translation, pp. 59-61).

So long ago as 1778 Captain Cook, writing of the shores of Bering Strait near Cape Denbigh, says: "After breakfast a party of men were sent to the peninsula for brooms and spruce. . . . It appeared to me that this peninsula must have been an island in remote times, for there were marks of the sea having flowed over the isthmus. And even now it appeared to be kept out by a bank of sand, stones and wood thrown up by the waves. By this bank it was evident that the land was here encroaching upon the sea, and it was easy to trace its gradual formation" (Cook's *Voyages*, 1842 edit., ii., p. 344).

Coxe, describing the voyage of Captain Kremtzin and Lieutenant Levashef in 1768-69, says: "The *St. Catherine* wintered in the Strait of Alaska and was drawn into shoal water. The instructions set forth that a private ship had in 1762 found there a commodious harbour, but the captain looked for it in vain. . . . May not this allow the conjecture that the coast had undergone considerable changes, even since the year 1762?" (Coxe's *Russian Discoveries*, p. 251).

Recently Russian travellers have discovered on the coast of the great island of Saghalien, north of Japan, heaps of modern shells lying not far from the shore in beds of marine clay, and also speak of former bays which are now converted into lakes or salt marshes.

In Whymper's account of his journey to Alaska I find him saying: "The island of St. Michael's is covered with moss and berries, resting on a bed of clay, but more commonly on a porous lava rock. The formation apparently extends to the Yukon. The Indians have a tradition that the island was upheaved from the sea, an occurrence at least possible. A large rock in the chain of the Aleutian Islands, known to the Russians as Bajaslov Volcano, rose from the sea in 1796." Zagoskin says that the spot where the fort (*i.e.*, Fort Yukon) now stands has been covered by the sea within the memory

of the Indians living at the date of his visit in 1842 and 1843" (*Journ. Roy. Geo. Soc.*, xxxviii.). Grant tells us that in Vancouver Island a raised sea-beach with scanty soil is mentioned as extending with a breadth of from 300 to 500 yards all along the north-east end of the harbour of Port St. Juan (ch. xxvii., p. 285).

This survey of the lands surrounding the North Pole makes it plain that the whole area is undergoing a general movement of upheaval, or rather, we everywhere find traces in all directions that there has been a movement of upheaval since there was any subsidence.

This continuous upheaval of land in the highest latitudes is only consistent, as I have argued, with a continued increase in the severity of its climate, and it is plain that since it began there must have been a progress there from a more temperate to a more severe climate, involving an increase in the amount of snow above the snow line, the growth of larger glaciers, the dispersal of larger icebergs, and the increase in the quantity of purely marine ice. Have we any direct evidence that such a climatic change in such a direction has, in fact, taken place? It seems to me that the evidence of this is plain and abundant. Let us first turn to Iceland.

That the climate of Iceland has been getting more severe was long ago suggested by the famous traveller Henderson. *Inter alia*, he says: "It is evident from ancient Icelandic documents that on the arrival of the Norwegians, and for several centuries afterwards, pretty extensive forests grew in different parts of the island, and furnished the inhabitants with wood both for domestic and nautical purposes. Owing, however, to their improvident treatment of them, and the increased severity of the climate, they have almost entirely disappeared."

Elsewhere we read: "In the Middle Ages the south-west parts of the island were covered with forests, and the old sagas report, without attributing anything extraordinary to the fact, that the house and boat builders went to cut logs in the adjacent forests" ("Suarfdvela Saga," A. Geffrey, *Revue des Deux Mondes*, 1st November, 1875).

Olafsson and Pälsson, who themselves saw a tree 12 metres high, heard speak of a ship built of oak which was launched

in the Hval Fjörde, north of Reykjavik, and thence went to Norway. These travellers in their travels found numerous iron forges, and it was probably the forgers who destroyed the forests (Reclus, *op. cit.*, iv., p. 925).

Mr. Watts, in his well-known journey across the Vatna Jokull, tells us that in passing through woods of birch and willow on the banks of the Grava lands, he noticed that the largest wood was dead. As on the upper fjelds in Norway trees will no longer grow where they once grew, and the fact is in all probability due to the general elevation of the country which has reduced its mean temperature and made its climate more harsh.

From Greenland the evidence is very curious and very interesting, since it seems only explainable as directly resulting from the law which I have striven to establish, namely, that the increase or decrease of the local glaciers, which is a measure of the severity of the climate, is dependent on the rising or sinking of the land.

As we have seen, there is an oscillation in Greenland in this respect. While to the north of the seventy-fourth parallel it is rising, to the south it is apparently sinking. Now, in the north of Greenland the glaciers seem undoubtedly to have been growing and extending over districts where formerly a more temperate climate prevailed, and notably in the days of the Norwegian settlement. The ice has in many cases encroached upon the dwellings and hunting-grounds of the Esquimaux, which are no longer habitable, and the skulls of the musk sheep, etc., have occurred where these animals can no longer find feeding ground.

"Of late years," says Nordenskiold, "the rowing of an umiak in Tessuvisarsoak has been rendered difficult by ice-blocks fallen from the glacier, which is said not to have been the case formerly; and one of our rowers, Henry Sissarmiak, even affirms that he rowed without obstruction seven years ago round an island which now forms a peninsula jutting out from the margin of the inland ice." Many similar examples in North Greenland are adduced. Thus, for example, the glacier that issues into Bläsedal, near Godhavn, has, since the time when Dr. Rink mapped that place, advanced much further into the valley; in the fiords around Omenak the ice

has advanced considerably within the memory of man; a path formerly often frequented between Sarfarfik and Sakkak is now closed by inland ice, etc. A similar case occurs in Jakobshavn. "In a word," he says, "this fiord is now filled throughout its whole length with huge icebergs, which completely close it even to kayaks. The shores of the fiord are therefore uninhabited and seldom visited. A tradition exists, however, among the Greenlanders that the fiord was in former times less obstructed by ice, and was consequently a good hunting and fishing place; and this is confirmed by the older maps of the fiord, but especially by the numerous remains of old dwellings which are still met with along the shores." He says "there can be no doubt that in many parts of North Greenland the inland ice is certainly gaining ground" (*Geol. Mag.*, 1872, pp. 367, 412).

"There is no doubt in my mind," says Kane, "that at a time within historical and even recent limits the climate of this region (*i.e.*, Northern Greenland) was milder than it is now. . . . The stone huts of the natives are found scattered along the line of the bay in spots now so fenced by ice as to preclude the possibility of hunting, and, of course, of habitation, by men who rely on it for subsistence. Tradition points to these as once favourite hunting-grounds near open water. At Renslaer Harbour, called by the natives Annatak, or 'the thawing place,' we met with huts in quite tolerable preservation, with the stone pedestals still standing which used to sustain the carcases of the captured seals and walruses. Sunny Gorge, and a large indentation in Dallas Bay which bears the Esquimaux name of 'the inhabited place,' shows us the remains of a village, surrounded by the bones of seals, walruses and whales, all now cased in ice" (*Arctic Explorations*, i., pp. 308, 309).

Kaluhmak, an old Esquimaux, told Hayes that it was a well-established tradition of their tribe that the Esquimaux once extended both to the north and the south, and that finally the tribe now inhabiting the coast from Cape York to Smith Sound were cut off by the accumulation of ice as well above as below them. The ice has accumulated in Smith Sound as in Melville Bay; and what were once evidently prosperous hunting-grounds, up to the very face of

Humboldt Glacier, are now barren wastes, where a living thing rarely comes. At various places along the coast Dr. Kane found the remains of ancient huts; and lower down the coast, towards the mouth of the Sound, there are many of more recent date (*ibid.*, p. 337). So much for the north of Greenland. The evidence from the south of Greenland, where the land is sinking, seems to be, as it ought theoretically to be, just opposite to this, and the glaciers there seem to be retreating. Let us move on, however.

Nordenskiold points to many circumstances proving that the glaciers in Spitzbergen have during the last few centuries not retreated but advanced considerably, as, for instance, at Horn Sound. That sound was apparently well known to the Dutch, as an old chart marks two anchorages there. They describe it as stretching one of its arms containing two islands somewhat northward, but at present this arm is occupied by an immense glacier, and excepting some small rocks there are no islands to be found in the bay.

A similar case was noticed by M. Robert at one of the arms of Recherche Bay (Bell Sound), and most likely analogous circumstances very much changed the shape of Stor Fiord, the bottom of which is occupied by an extensive and low glacier stretching in an even slope as far as Mount Chydennis. The large islands which, according to old charts, were situated at the inner extremity of the bay, cannot be the same small islets that are now to be found there, and it seems more probable that Mount Golland, now encompassed by glaciers and some other neighbouring mountains similarly shut up by ice, were at the time the whalers visited them surrounded by water, and identical with those islands which on the old charts are called Sea Horse Island and Seal Island. "At Bell Sound I myself witnessed a most striking proof of glaciers thus descending upon tracts hitherto free from ice. On the north coast of Bell Sound, directly to the east of the large island which separates Van Nugens Bay from the main bay, there existed only a few years ago one of the best harbours in Spitzbergen. The whalers on their way from the north coast to Stor Fiord used often to anchor at this harbour in order to hunt reindeer in the neighbouring fertile valleys, and this too was one of the first places visited by Prof. Torell's expedition

in 1858. . . . During the winter of 1860-61 the previously insignificant glacier descended upon the lowland, and the great hillock on the shore filled up the harbour and extended far into the sea. It now constitutes one of the largest glaciers of Spitzbergen, from which immense blocks of ice constantly fall down, so that not even a boat can venture in safety beneath its broken border" (*Sketch of the Geology of Spitzbergen*, pp. 8, 9).

There is clear evidence everywhere that in Spitzbergen where the land has recently risen the climate has become more rigorous.

At every point, therefore, where we can test them, the facts point unmistakably to the mean climate of the Arctic lands having been at all times a function of the elevation of its land masses above the sea-level, and to its present severity being due to that elevation being now excessive, and, further, that this elevation, which is still in progress, has, according to all the evidence, been continuous, and is the last movement which has taken place there, and that coincidently with it there has been a continuous increase in the rigour of the Arctic climate, and with it probably also an increase in the rigour of the climate of temperate latitudes. The facts above quoted, instead of pointing to the Arctic regions having escaped from more glacial conditions and gradually become more temperate, point unmistakably the other way.

If, then, there ever was a glacial period in the circumpolar lands, such as the glacialists affirm, it must have been not only before the latest rise of the land began, but before it sank to the position from which it is still rising, and when it must have been at a much higher elevation than it is now. Of this former elevation we have, so far as I know, no evidence whatever. Chamberlin's repeated statements to the contrary are mere *obiter dicta*. It would seem, in fact, that until the current geological period there never was such an elevated land in the North Polar region as we now find there, nor anything like the rigorous climate that prevails there now. If this be so, it means that the Arctic lands furnish us with no *a priori* evidence that a so-called glacial period ever existed there at all, unless we can call the present and current period a glacial one. Let us now examine the more direct witness they give us in this behalf. We will first turn to Iceland. The moun-

tains of Iceland where free from snow seem to me to unmistakably prove that there never was a great mass of ice there occupying the high ground and polishing and smoothing the surface. The contour of the mountains is ragged and torn and splintered, and in some places, such as near Allmanna-Gia, it is cut into the most fantastic shapes with projecting pinnacles.

Again, the many blocks of stone found there on the surface of the rocks are not strangers and true erratics, but are all natives. Again, as Mr. Baring Gould, who has travelled much there and has written a graphic account of the island, says: "There are no traces of moraines there except at the skirts of modern glaciers". He adds that only in one spot had he found unmistakable glacial grooving, namely, along the hill above Biarg in Mith Fiord, and he points out that what are really the results of the action of flowing water in polishing the surface had been mistaken by some for the action of ice. Thus, in describing the so-called heithis or fell lands, the same writer says: "These heithis being exposed to the action of snow water are much torn and mangled, the rock being, in many places, quite polished by the streams, from the thawed snows, as they slide over them. Mr. Chambers, in passing this same tract of moor from a different direction, saw similar polishings, and at once put them down to glacial action, and the furrows caused by the little rills to the striæ of glacial grooving. I believe him to have been mistaken, for the following reasons: the rock is not smoothed except where the streams flow over it, and a slight node of rock three inches high is quite sufficient to divert the striæ and alter the direction of the polished surface. A considerable removal of earth had taken place this spring, and I observed no marks of glacial smoothing on the rock upon which the soil had rested; it was ribbed and curdled like ordinary lava" (*Iceland, its Scenes and Sagas*, p. 63). Mr. Howell, in a communication to the *Geological Magazine*, in referring to the arguments here used, and which I had quoted in that periodical, says: "Having travelled through the country twice (1890 and 1891), mainly in quest of the summit of the highest peak, I have had good and numerous opportunities of observing the peculiarities of its mountain structure. On the whole, there can be no doubt as to the general result being decidedly in favour of Sir

Henry's views." After some sentences in which he somewhat minimises the evidence I quoted, Mr. Howell proceeds: "It is of the utmost importance to notice that among the hard basalt masses of the east and south-east the lower ridges, up to the height of about 1,000 or 1,500 feet, are often rounded, and striations are not uncommon, but above that height, at elevations which increase as one travels inland, these rounded hill-tops disappear, and sharp pinnacles form the rule. So sharp are these pinnacles that it takes a stranger some time to realise that they are not columns built upon the mountain summits. . . . There is a far older and safer guide telling of a time when many of these valleys up to a certain level had their glaciers, which, however, never formed part of an ice-cap to the island. So marked is the distinction between these peaks, rounded and pinnacled, that the conviction I have stated fairly forces itself upon one.

"Further west along the south coast evidence of another kind is forthcoming from the volcanic masses which there replace the basalt of the south-east and north-west. So soft are many of the ridges that, on the hypothesis of a huge grinding ice-cap, they ought to have disappeared altogether. Yet there they remain, loose, shaly, laminated ridges between which, in the valleys, lie enormous snow-fields, with drainage glaciers coming down to the sea-level now and overtopping lower hills, just as the more easterly ones used to do. On every hand signs abound, both of advance and of retreat, on the part of glaciers accommodating themselves to the inequalities of their beds, but none whatever of ice above a definite ascending level, nor of excavations below that level" (*Geol. Mag.*, 1893, p. 426).

If we turn from the mainland of Iceland to the islets and rocks which surround it the evidence is the same. These islets stand up in the form of splintered pinnacles and needles, and form the best possible evidence that ice cannot have smothered and smoothed the face of the country. Thus Baring Gould says: "Rock needles, which abound on the coasts, are named drangir by the natives. Some of these are very noble. The entrance of the Isa Fiord is guarded by one such standing up from a platform of basalt high above the water; it goes by the name of 'the Sentinel'. A curious

spire of rock above the Hörgärdale is illustrated on plate xii. There are needles in the Skaga Fjord off Drangey, and in the Breithi Fiord" (*op. cit.*, xxix.).

The existence of these pinnacles and sharp-angled rocks is inconsistent with the former existence of a great glacial covering. They would surely have been ground down and swept away if a great mass of ice corresponding to that which we postulate, when we speak of an ice age, had formerly prevailed. The evidence of the physical features of the country is amply supported, as it seems to me, by the biological evidence. Iceland is too far removed from any other country to have been peopled in regard to its fauna and flora by scattered and occasional migrants. It is incredible that its mammals, its sedentary birds, its fresh-water fishes and its insects could have reached it in this way, much less its plants. It seems impossible to believe that its comparatively rich flora is the result of sporadic colonisation. Long ago Pennant wrote in his ingenuous way that there are found in Iceland 309 perfect and 233 cryptogamous plants, while on the Island of Ascension, which is totally and aboriginally volcanic, a flora of not more than seven plants is to be seen. This means that either Iceland has been connected with Europe by a land bridge since so-called glacial times, which is almost incredible, or that its flora survived the so-called glacial age, which is much more probable, and which means that nothing like the glacial period of the glacialists ever existed there. It is clear in every way that Iceland furnishes no evidence in favour of a former ice age in which the land was covered with an ice-sheet and life was exterminated, but just the contrary. It may have been that its glaciers have had an ebb and flow in their growth, but of "an Ice Age" there is no evidence there at all, and surely this is the place whence, in Western Europe, such evidence should be forthcoming if an ice age ever existed at all. Let us now turn to Greenland.

The question in regard to Greenland was formerly complicated by an arbitrary opinion published by Dr. O. Torell (by no means the only rash and mischievous publication on the subject of glacial matters emanating from that influential geologist). He expressed the view that a large number of the boulders and erratics found in North America came from

Greenland (see Torell on "The Causes of Glacial Phenomena in the North-east Portion of North America," *Sweriges Geol. Under.*, 1878, pp. 6, 7). As a matter of fact this *obiter dictum* of Prof. Torell has been shown to have been entirely without foundation, and so far as is known now not a single stone from Greenland occurs among the erratic blocks of North America, nor outside of Greenland. Let us turn to other of its features, therefore.

Greenland is now so overwhelmed with ice that even if it had had a former so-called glacial period we should not be easily able to disentangle and separate its traces from those of the Arctic conditions which now prevail there, so that we cannot adduce evidence there of the same kind as we can elsewhere, namely, old polished and striated rocks, boulders, etc. It is, of course, very well known that in Greenland there are undoubtedly considerable traces of polishing, scoring, etc., outside the range and reach of the living glaciers. Thus Nordenskiold refers to the extensive rounded, polished and grooved border of the land which almost everywhere separates the inland ice from the extreme coast; and he goes on to argue that this points to a comparatively recent retirement of the ice, and he adds, in addition, that none of the numberless small sea basins in North Greenland, in spite of the suitableness of the locality for moss vegetation, have yet become filled with turf even to the depth of a few feet, showing, to use his own words, "that the slip of ice-free land is but a child of yesterday". I admit completely Nordenskiold's facts, but I cannot follow his inference. As every one knows, the coast of Greenland north of a certain latitude is rapidly rising from the sea, and the rounding and polishing and striating of the marginal land was, it seems to me, the result not of the action of greatly increased land ice, but of that most effective scraper and polisher, *either* shore ice, acting as it still does on this coast and also on the coast of Labrador, or the waves themselves when armed with shingle or other abrading tools. Let me, *apropos* of this, quote an apposite sentence from Hayes.

Speaking of the rise of the coast from Cape York northwards, he says: "At many conspicuous points where the current is swift and the ice is pressed down upon the land

with great force and rapidity, the rocks are worn away until they are as smooth and polished as the surface of a table, a fact which may at any time be observed by looking down through the clear water. This smoothness of the rock continues above the sea, to an elevation which I have not been able with positive accuracy to determine in any locality, but having a general correspondence to the height of the terraces at Port Foulke, which rise 110 feet above the sea-level. At Cairn Point the abrasion is very marked, and, where the polished line of syenitic rock leaves off and the rough rock begins, is quite clearly defined. The same condition also exists at Littleton Island (or rather McGary Island, which lies immediately outside it) to an almost equally marked degree" (*Open Polar Sea*, p. 403). This is a very interesting and far-reaching induction, and it seems to me quite plain that as in the case of the hundreds of rounded and hogbacked and polished surfaces of the Arctic archipelago and other northern lands, which we have abundant evidence have only recently emerged from the sea, their rounded and polished surfaces have nothing to do with ice-sheets, but are simply caused by tidal erosion when the tide is armed with shingle or shore ice. Let me support my view by that of two of my acute friends.

Speaking of Grinnell Land, De Rance and Fielden say: "The rock surfaces at considerable elevations, between gaps in the lines of old terraces, are often found to be glaciated; and there can be little doubt that this glaciation was produced by shore ice, during ebbing of the tide, when the land stood lower than at present; and the condition of the terraces precludes the idea of glacier action. . . . Sea ice moved up and down by tidal action, or driven on shore by gales, was found to be a very potent agent in the glaciation of rocks and pebbles; the work was seen in progress along the shores of the Polar basin at the south end of a small island in Black-cliff Bay (lat. 82° 30′ N.). The bottoms of the hummocks, some 8 to 15 feet thick, were studded with hard limestone pebbles, which, when extracted from the ice, were found to be rounded and scratched on the exposed surface only" (De Rance and Fielden, *Nares' Narrative*, ii., p. 343).

Fielden similarly attributes the polishing and rounding of

the present coast line and islets of Waigatz to the action of pack ice loaded with gravel and stones, helped by wind and tide. "On examining," he says, "the points of contact between the ice and the islets, we found the rocks both below and above the tide line highly glaciated and scored, whilst the sides of the narrow gaps between the islets showed similar results. These marks of ice action are obviously produced by the floating ice, because higher up the sides of the islets they decrease in intensity, and on their summits are nearly effaced through the decay and splintering of the rock itself. Had this rock-grinding been the result of a former ice-cap, we might with good reason expect to find it as much in evidence on the higher ridges as at the present shore line. On the east side of Waigatz, exposed to the full weight of the Kara Sea pack, the shore line of rocks is intensely glaciated; mounds of gravel and rocks with drift-wood thrust up many feet beyond tide line testify with what prodigious force the shore ice is propelled. The basement rock over which these mounds of gravel and stones have passed are polished and striated. . . . At Cape Matiuska the boulder clays and mud and sand beds reach the imposing thickness of 100 feet or more of marine boreal deposits. I therefore find it difficult to escape from a process of reasoning which ascribes a common origin to phenomena visible on the land, and to similar ones now in progress at the shore line and in the surrounding sea" (*ibid.*, pp. 270, 271).

It is almost certain, therefore, that the polished and rounded surfaces of the sea-board of Greenland have resulted not from the polishing of land ice, but are the result of submarine erosion. This is not all. On this matter I wish to quote an extreme glacial champion, Mr. Chamberlin. "The amount of drift on the territory once occupied but now free from ice," he says, "is notable rather for its scantiness than its abundance. On Disco Island it was found to be very limited except along the immediate fronts of the present glaciers. In the Inglefield Gulf region there are at some points very considerable accumulations of drift within a mile or two of the ice-front, and the sea bears a very scant covering of drift. No great moraines were seen nor any thick mantles of drift. The valleys in front of the glaciers are well-

floored with glacial wash, but even here the rock occasionally appears. Considerable delta-fans project into the gulf, but none of them exceed half a mile in depth. Consonant with this scantiness of drift the topography of the borderland shows only moderate evidence of glacial subjugation. It is mildly rounded but not greatly moulded.

"Several glaciers on Herbert and Northumberland Islands showed evidences of retreat; the terrace-like pedestals which they had formerly built were in part abandoned. Three other glaciers showed by the presence of old moraines immediately in front that in the past they had been more extended than at present. These moraines may be a few hundred years old, but they offered no evidence of very great antiquity. One glacier was seen overriding its terminal moraine in one portion and retreating within it at another. This, taken in connection with the massiveness of the moraine, probably indicates that it has stood practically stationary for a considerable period.

"The most remarkable evidence relative to former extension is furnished by a driftless area on the east side of Bowdoin Bay, immediately adjoining the present great ice-cap. It is obvious that at this point the ice is as far advanced as it has ever been in the recent geologic history of Greenland. The verity of this driftless area is attested not only by the absence of transported material upon it, but by the exceedingly angular ragged disintegration of the harder terraces of rock embraced in the complex gneissic series, and by the deep disintegration of the gneiss itself. The gullies and ravines reveal the fact that the gneiss is deeply decomposed to a soft rotten mass, which is not only easily crumbled but is thrusting the heels deep into the softened mass. The combined weight of all this evidence puts beyond serious question the verity of the driftlessness of this region. The area is small, not exceeding three or four miles in maximum diameter, and lies between the ice edge and Bowdoin Bay on ground whose average altitude is less than that of the glacier, so that its immunity from glaciation has not been due to elevation. It is clear, therefore, that the ice border was stayed at this point by agencies concerned in its own development, and not by any topographic barrier.

"Immediately at the south of this small driftless area there lies in front of the Gable Glacier (which is but a short tongue of the main sheet) a stout old moraine, the surface of which has been notably weathered, and has become covered with vegetation in the scant fashion of the region. There is nothing in the nature of this moraine to indicate an antiquity beyond perhaps a few hundred years, but its presence at this point seems to indicate that the ice has stood in the vicinity for a considerable period, and, therefore, that it is probably on the average neither much advancing nor much retreating.

"It is evident that the occurrence of even a small driftless area on a border of the widest stretch of the Greenland ice-sheet is extremely significant, restricting its former extension. The general scantiness of the drift over the territory immediately outside of the present ice seems also to raise doubt as to any former extension. There are two other lines of important evidence that bear upon this question. Dalrymple Island is a mass of hornblendic gneiss rising from the water's edge to a height of perhaps 100 feet, with steep slopes and ragged surfaces. It is a famous nesting-place of the eider-duck, which finds it suitable to its purpose because of this raggedness of surface. The island bears no sign of glacial abrasion. It stands at the mouth of Wolstenholme Sound on the west coast in about latitude 76° 50′. In other words, it is just off the border of one of the broadest stretches of Greenland's ice-field. Thirty or forty miles distant to the west-north-west lie the Cary Islands, which are formed of almost identical rock. They are very notably abraded at heights of 500 feet above the sea. There also occur upon them erratics of limestone, sandstone, shale and quartzite, wholly unlike anything that occurs in the islands themselves. So far as I know no rock of similar kind occurs in Greenland to the eastward. These erratics appear to have come from the region beyond Smith's Sound to the north either from Grinnell Land or from the north-western coast of Greenland, more likely the former than the latter. It appears, therefore, that while a very notable southerly movement from the far north took place down the valley and reached at least to the Cary Islands, there was no corresponding movement from the east.

"At the very first glimpse of the coastal mountains of Southern Greenland, I was impressed by their pronounced angularity, and the absence, unless it were in the lower valleys, of any notable signs of the horizontal rasping which must have resulted had the inland ice ever pushed across them into Baffin Bay. Subsequently I saw approximately a thousand miles of coast line, and an effort was made to discriminate the portions once overridden by ice from those which had not been. Tracts of angular unsubdued topography were found alternating with tracts of rounded, flowing contours. About one-half of the coast seemed to belong to each type. The inference was drawn that the ice formerly so extended itself as to reach the present coast for about half of its extent, while in the remaining portion the ice fell short. Combining this topographic evidence with the specific data furnished by a comparison of Dalrymple Island with the Cary Islands, and with the still more stubborn facts of the driftless area of Bowdoin Bay, the inference seems unavoidable that the ice of Greenland on its western side at least has never advanced very greatly beyond its present border in recent geologic times. This carries with it the dismissal of the hypothesis that the glaciation of our mainland had its source in Greenland." Chamberlin goes on to postulate a former greater elevation of Greenland without adducing any evidence, and in fact quite arbitrarily. What is important to remember is the collection of facts he quotes, and which he again refers to as proofs that there has not been a real glacial period in Greenland, thus: "The contours of the plateaus and valleys which seem to indicate a fashioning rather by meteoric agencies than by pronounced glaciation and the driftless area appear to afford the most specific ground for induction". He bids us also bear in mind "that this is a small area between the present edge of the ice and sea-level which would be overridden easily and completely by an advance of the ice edge of less than five miles. . . . The raggedness of Dalrymple Island bears similar testimony. The general angularity of the coastal mountains of South Greenland throw the weight of their evidence in the same direction."

Chamberlin's arguments can be supported in other ways, thus:—

The presence of pinnacles and pillars of rock standing up with a perfectly free space round them, seems here, as in Iceland, to be quite inconsistent with the former existence of an ice-sheet on a much greater scale than now exists. Kane speaks of three pillars of greenstone which he calls "Three Brothers' Turrets". These are situated near Dallas Bay. At Sunny Gorge, again, north of 79°, he speaks of a solitary column or minaret tower as sharply finished as if it had been cast for the Place Vendôme; the length of the shaft is 400 feet, rising on a plinth itself 280 feet high, and he named it "Tennyson's Monument".

This being the evidence on the coast, we can supplement it by evidence of another kind from the interior of the country. The great sea of inland ice in Greenland is dotted with islets of rock, etc., which are known to the Greenlanders as *nunataks*. The unweathered and rugged outlines of these projecting rocks show they have never been smothered by ice, and show, therefore, that the ice of Greenland has reached its culminating point quite recently, and was never more developed than it is now, for if it had been so these *nunatakker* would have been rubbed down smooth.

They are, however, remarkable witnesses for our contention in another way. Quite a considerable number of plants have been collected from these *nunatakker* by the Danish botanists, and as the *nunataks* are isolated and occur sometimes at great distances apart, I know of no explanation which will account for this flora except that the *nunataks* instead of having been at no remote times portions of a continuous ice-covered land surface, were in fact parts of a land surface on which much more moderate conditions prevailed.[1]

It seems to me quite impossible, again, that these rocks, situated as they are in the midst of the Greenland ice-plateau, could have got their animal and vegetable life from any occasional waifs. The lessons of the Greenland flora are most pertinent in this behalf.

In 1861 Sir Joseph Hooker wrote his famous paper on the distribution of Arctic plants, in which he argued that the flora

[1] In my paper on this subject, from which the above paragraph is quoted (*Geol. Mag.*, 1893, p. 306), I have entirely confused the sense of this statement by an inconceivably stupid sentence.

of Greenland is distinctly of European type; that Greenland is a sub-region in fact of the Scandinavian botanical province. To state his own conclusion in his own words: "The flora of Greenland is almost exclusively Lapponian, having an extremely light admixture of American or Asiatic types". This view had already been maintained by Charles Martins in 1839, although he had not the same materials to work upon. Speaking of the plants of Shetland, the Faroes, Iceland and Greenland, he says: "La migration européenne est évidemment prédominante". Hooker's view was maintained by Blytt, who, writing in the *Journal of Botany* for 1887, says: "Even the Greenland flora consists principally of Scandinavian plants". Sir Joseph Hooker, further, concluded "that the existing Scandinavian flora is of great antiquity; that previous to the glacial epoch it was more uniformly distributed over the polar zone than it is now; that during the advent of the glacial period the Scandinavian vegetation was driven southwards in every longitude, and even across the tropics into the south temperate zone; and that, on the succeeding warmth of the present epoch, those species that survived both ascended the mountains of the warmer zones, and also returned northwards, accompanied by aborigines of the countries they had invaded during their southern migration". He further says: "If it be granted that the polar area was once occupied by the Scandinavian flora, and that the cold of the glacial epoch did drive this vegetation southwards, it is evident that the Greenland individuals, from being confined to a peninsula, would be exposed to very different conditions to those of the great continents. In Greenland many species would, as it were, be driven into the sea, that is, exterminated, and the survivors would be confined to the southern portion of the peninsula. Not being there brought into competition with other types, there could be no struggle for life among their progeny, and consequently no selection of better adapted varieties. On the return of heat these survivors would simply travel northwards, unaccompanied by the plants of any other country."

The general conclusions of Hooker and others about the affinities of the Greenland flora, and the widespread induction based upon them, have been recently sharply contested by two Scandinavian botanists. In 1880, Joh. Lange, in his

Conspectus Florae Grönlandicae, came to the conclusion that out of 386 species of Greenland plants fifteen are endemic, forty belong to the Western or American, and forty-four to the Eastern or European district—that is to say, are otherwise only found in those districts; or, interpreting the facts most favourably to the eastern side, there would be thirty-six western as against forty-two eastern, a majority of six only in favour of the latter.

Some years later Warming subjected the botany of Greenland to a minute analysis, and came virtually to the same opinion as Lange. He divides Greenland into two botanical provinces—a birch region (*R. sub-Alpina*) and an Alpine region (*R. Alpina*). The former is limited to the extreme south of Greenland, and is bounded by a line from Cape Farewell, 60° N. lat. on the east coast, to about 62° on the west, and contains about sixty species of plants not otherwise found in Greenland. The other region includes all the rest of the country. His conclusion is, that while the former small district contains a considerable number of specially European types, the latter, *viz.*, Greenland proper, contains hardly any, but does contain a number of American ones. The great bulk of the plants, however, of Greenland belong neither to Europe especially nor to America, but are distinctly circumpolar. Its fifteen endemic plants consist of six species of *Carex*, three of *Potentilla*, *Epilobium ambiguum*, *Arabis Breutelii*, *Campanula grönlandica*, *Calamagrostis hyperborea*, *Glyceria langeana*, and *Poa filipes*.

Of the specially European plants, most of which are only found in the extreme south of Greenland, Warming considers that a large proportion have found their way there since glacial times either by ocean currents, by being carried by birds, and in other ways. It would seem most certain, in view of his very careful analysis of the problem, that Hooker's original view can no longer be maintained, and that Greenland cannot be treated as a section of the Scandinavian botanical province. Nor can we fairly treat it as a section of the North American botanical province either. On the contrary, it seems plain that it forms an integral part of what ought to be discriminated as a Pan-Arctic or circumpolar botanical region.

This raises some very important questions, and notably

those connected with the ice age. The notion that the flora of Greenland was virtually exterminated by the ice age and has found its way thither again since that period must go to the wall. As Warming emphatically urges, and we cannot avoid his conclusion, the present flora of Greenland outlived the so-called glacial age there. Warming says, further, that Greenland during the ice age clearly had ice-free land. The many Danish geological and geographical expeditions which during the last ten years have visited the country have mapped it and described its geology and botany, and have made it plain that in South Greenland, above the height of 2,000 to 3,000 metres, there is no mountain top showing any signs of former ice action. This would include several isolated mountains along the west coast, as far as 70° of N. lat. and a large part of the uplands of South Greenland. Warming emphatically says there is no trace here of a general ice-covering, but only of local glaciers; it is here, he urges, that the plants we are discussing continued to live. Another similar vantage for them was in North-east Greenland, in the neighbourhood of Franz-Joseph Fiord, where also there is evidence that the land was not mantled with ice. Similar conditions also probably prevailed in Grinnell Land, where Greely, and about Discovery Bay in 82° 44′ N. lat., where Nares, found a comparatively rich vegetation with several very local species.

Warming further urges that the survival of the present flora of Greenland from pre-glacial times is also attested by the rarer plants. Putting aside South Greenland, where a considerable number of the plants may have been imported in post-glacial times, the second German polar expedition found a number of plants in North-east Greenland such as *Polemonium humile, Arabis petræa, Saxifraga Hirculus* and *hieraciifolia, Draba altaica, Ranunculus glacialis*, etc., which are either not found elsewhere or in very few places. "It seems to me probable," says Warming, "that most of these, probably all, are to be numbered among the Autochthones." In North-west Greenland we find such rarities as *Pleuropogon Sabinei* and *Hesperis Pallasii*, which also probably outlived the ice period. *Androsace septentrionalis*, which the Nares' expedition found in Grinnell Land, is removed by a long distance from where

it otherwise occurs. On the west coast are other rare plants, as *Ranunculus glacialis* south of Upernivik; *Eutrema Edwardsii* in latitude 70° 47′; *Taraxacum phymatocarpum* in 70° 74′ N. lat.; *Utricularia minor* from 68° 21′ to 69°, elsewhere it occurs in the Urals and the Altai. *Scirpus parvulus*, found in two places between 68° and 69° N. lat., also European; *Cerastium arvense* found once in 67° 5′ N. lat.; *Carex helvola*, also found in 67°, and also European; *Linnæa borealis*, found in 1883, near Ivigtut in 61° 10′, and in 1884 about 67°. *Arctostaphylos alpina*, sparingly between 70° and 65°; *A. uva ursi* only in one place, about 67° N. lat., near Holstensborg; *Sisymbrium humile*, found in 1884 in the upper part of "Sondre Stromfjorde," in 66° 30′ N. lat. Then among plants only found otherwise in America and West Siberia: *Gentiana tenella*, found in 1889; *Vahlodea atrapurpurea, Andromeda polifolia, Rubus chamæmorus*, only found in Godhavn in 64° 10′ N. lat., and several others. "The distribution of these plants," says Warming, "seems to me to point to their being relics of the old flora which have survived in favourable localities." He further argues that while some plants have managed to survive, others have succumbed, and notably such widespread genera as *Chrysosplenium* and *Caltha*, and also the various genera of *Oxytropis, Astragalus, Phaca*, etc., which are so widely spread in Alpine districts, and also in the Arctic regions, but are not found in Greenland. One of the most notable absentees from Greenland is the *Salix polaris*, which occurs in Spitzbergen, Scandinavia, North Russia, Siberia, and North America as far as Davis Straits.

The evidence of the plants is confirmed by that of the shells in the raised beaches. Thus, we read of the pleistocene shells in the clay beds at Sarpiurssak in Greenland, and the remains of fish from the same place, discovered by Dr. Rink. "A collection of them was sent home and examined by Dr. O. A. Mörch, who found that the molluscs consisted partly of shells still living on the Greenland coast and partly of molluscs which now live only in more southern latitudes."

De Rance and Fielden remark on the continuity of the shells in the mud-banks of that very northern land, Grinnell Land, with those in the neighbouring seas, and the occurrence in the same mud-beds of bones of the lemming, ringed seal,

reindeer and musk-ox as proving that the conditions there were never so rigorous as to preclude the existence of such animals, and they add what Nordenskiold and others have also said: "We think it worthy of note that no records of former glacial episodes have yet been discovered in the polar lands".

In regard to Greenland, therefore, we must accept the same conclusion as to Iceland. All the available evidence goes to show that in the so-called glacial age the climate instead of being more severe was milder than it is now.

The conclusions of Warming and others about the flora of Greenland may be paralleled by those of Nathorst in regard to Spitzbergen. He also concludes that the flora of that island, instead of being an importation since glacial times, is really the wreck and ruin of what was once a much richer flora which has been able to survive the drastic conditions which now prevail there.

The evidence of the flora is confirmed by other facts in Spitzbergen, *e.g.*, by the fragments of marine shells, bones of whales, etc., met with in the raised beaches on many parts of the coast of that island. Thus Keilhau mentions that at Whale's Point, far from the shore and at a height of about 100 feet above the present sea-level, he found sub-fossil shells of the same species as those still occurring on the coasts of Norway.

This means, surely, that the seas round Spitzbergen have become colder since some of the raised beaches were deposited. The testimony of the fauna and flora of the archipelago of Spitzbergen is similar to that of Iceland and Greenland. We cannot conceive its having reached there except by a land bridge, and for such a land bridge we must go much further back than the so-called glacial age. The presence of reindeer of a specialised form in Spitzbergen is a strong proof of this, and it shows that nothing like the ice monster of the glacialists has laid its load upon these lands, whose desperate condition is the growth of comparatively recent times.

This is also supported by the more purely physical evidence. Spitzbergen bears in its very name the evidence of the sharply outlined needle-like contour of some of its mountains. In the

journal of William Bernard, the companion of Barentz, who discovered it, we read: "La terre estoit la plus part rompue bien hault et non autre que monts et montaignes agues parquoy l'apellions Spitzbergen" (Reclus, *op. cit.*, v., p. 257).

Heughlin describes its west coast as a congeries of deep fiords, and the mountains, to use his own words, as "nicht gesonderte Massen mit vielzackigen spitzigen Gipfeln auftreten" (*Petermann's Mittheilungen*, 1871, p. 177). Elsewhere the same writer compares the rugged and fantastic outline of the islands with the crenellated outline of a mediæval castle.

This is assuredly quite inconsistent with Spitzbergen having been smothered with ice, as it should have been in the high latitudes where it is placed, if the so-called glacial age had ever existed there.

This is the view of the best observers. Thus Garwood and Gregory, writing on Spitzbergen, say:—

"The sharp, serrated ridges of Mount Staraschin and Dodman Den, which guard the entrance to the ice fiord, indicate that Western Spitzbergen has not at any recent time been wholly submerged beneath an ice-cap" (*Quart. Journ. Geol. Soc.*, liv., pp. 197, 198).

"Though I had an opportunity," says Nordenskiold, "of examining in several places in Spitzbergen old beds of glaciers surrounded by solid rocks, I have only very seldom met with any rocks polished and furrowed by glaciers, and those were besides situated on the very edge of the sea. Such extensive and highly polished surfaces of rock as are everywhere met with in Scandinavia are not to be found there" (*Sketch of the Geology of Spitzbergen*, p. 7, note).

The presence of polished rocks close to the sea-level in a country which is rising from the waters so rapidly as Spitzbergen, really points there, as we have seen it does in Greenland, not to old glacier action but to the scoring of recent shore-ice. Thus may also be explained the polished surface of the low-lying Deuen Islands, and which have probably risen from the sea quite recently. The débris of pleistocene marine life in Spitzbergen such as it is points the same lesson, and Nordenskiold distinctly suggests that in the interior of Ice Fiord and at several other places in Spitzbergen one meets with indications that the polar tracts were less completely

covered with ice during the glacial era than is usually supposed (*Geol. Mag.*, 1875, p. 531).

Nordenskiold says: "Our investigations at Spitzbergen throw no light on the transition from the Miocene period to the glacial formation of the present time, or on the nature of the animals and plants that lived in the vicinity of the Pole during the Pliocene and the European glacial periods. We did not there meet with any deposits that might be regarded as a link between the tertiary strata at Bell Sound and the present age. Torell and Malmgren found on the shores of Hinlopen Sound and Blomstrand, in an especially interesting layer of earth on the shores of Advant Bay, shells of *Mytilus edulis*, which occur abundantly along the Scandinavian coasts, but no longer inhabits Spitzbergen; at least the zoologists of the Spitzbergen expeditions were not able to obtain a single living specimen of it in the dredgings undertaken by several boats almost every day during three summers in the bays and along the coasts of Spitzbergen. Immense numbers of this shell still inhabit the shallows and banks laid bare by the tide at Tromsö and Hammerfest" (*Sketch of the Geology of Spitzbergen*, pp. 52, 53). This assuredly points to the climate having become more and not less severe since the shells were deposited.

It seems to me, again, that we cannot account for the fauna and flora of Spitzbergen on any other theory than that we have already applied to Iceland and Greenland, namely, that it marks the shrunken relics of what was once much richer.

If we lastly turn to Franz-Joseph Land we shall arrive at the same conclusion. Payer describes its rugged outlines, its rows of sharp basaltic columns, its jagged line of mountains towering above the snow-fields, and he figures in more than one place ragged and torn and pinnacled rocks which are not consistent with the land having been abraded and denuded by an overwhelming ice-sheet, and point, in fact, to the present conditions being the climax of the ice action there. Payer remarks on the infrequency of moraines, and tells us, further, that however diligently he looked for them he never saw unmistakable traces of the grinding and polishing of rocks by glacier action there.

Koettlitz says of the same archipelago: "The evidences

of glacial action as shown by the smoothing and planing of rocks are very few; *roches moutonnées* and rounded hills have not been met with. Only in two valleys, separating Cape Flora and Cape Gertrude, are some loose blocks of basalt which are planed, polished and somewhat scratched on their upper sides. . . . They are upon low and recently raised beaches. . . . No evidence has been obtained of the existence of anything that could be definitely called till" (*op. cit.*, p. 644).

All this surely points the same moral, namely, the absence from the very Arctic country of Franz-Joseph Land of any adequate evidences of the so-called Ice Age.

Novaya Zembla gives us a similar answer. Fielden, speaking of the mountains there, says: "I can see no evidence of the former extension of an ice-sheet over the area, no *roches moutonnées*, no glaciated surfaces, no rounded, mammillated, ice-worn contours. Had these ever existed one can hardly suppose that they would have been totally removed by the action of frost" (*Quart. Journ. Geol. Soc.*, lii., pp. 731, 732).

Turning to the northern coast of Siberia, Nordenskiold tells us of the Bear Islands. They are formed for the most part of plutonic rocks, whose upper part has weathered away, leaving gigantic isolated pillars. Four such pillars have given to the easternmost of the islands the name of Lighthouse Island.

Similar ruin-like formations are found not only on Cape Baranof, which lies opposite, but also at a great number of places in that portion of the north coast of Siberia which lies farther to the east. "Generally these cliff ruins are collected together over considerable areas in groups or regular rows. They have thus, when seen from the sea, so bewildering a resemblance to the ruins of a gigantic city, which had once been surrounded by strong walls, and been full of temples and splendid buildings, that one is almost tempted to see in them memorials of a Tamerlane or Chinghiz Khan up here in the far north" (*Voyage of the Vega*, i., p. 428). These pillars and prominent unweathered rocks in the very high latitudes of Asia are also mentioned by Baron Toll and Professor Bunge in the account of their recent expedition to the Bear Islands. As they lie in the very track of a great polar ice-cap, if such had ever existed, we cannot understand how they could escape having been broken or entirely worn away.

The evidence furnished by the presence of the mammoth and its companions in huge herds buried in the tundra of North Siberia and among the boulders and ice blocks of the New Siberian and Bear Islands in the Arctic seas of Asia points the same irresistible lesson. As I showed by ample evidence in my work on the mammoth, these beasts lived where their carcases and skeletons are now found, and must have had abundant supplies of trees and shrubs to feed upon since the contents of their stomachs prove it, and such a vegetation must have flourished many degrees to the north of the present range of trees in the very last geological period. This biological evidence is completely consistent, of course, with the fact that even the champions of the glacial period exclude its operation from Siberia, and argue that traces of its operation are not to be found between the White Sea and the River Mackenzie. It is further interesting to find that in Alaska where the same biological evidence is forthcoming in regard to the mammoth and its companions, the plant remains found growing on the moraine of the retiring glacier at Mount St. Elias confirm the same position as do the plants of Greenland. All this converges upon one conclusion, namely, that if there has ever been a so-called glacial period in the Arctic regions it is now. The present period is the one period and apparently the only period in their history when the reign of ice and snow has been dominant. So far as we can make out, their climate has been becoming more and more severe in historic times, and in the pleistocene age, so far as we have evidence, the climate of the polar area was a great deal more and not less temperate than now. All this is completely at one with the more recent speculations of the zoologist, who sees in the polar area the centre and focus of a geographical region in which the animal forms are alike in all meridians, namely, the so-called circumpolar area, and who further argues that the two provinces which are known as Palæarctic and Nearctic, in so far as they are differentiated from each other, have become so very recently, and that in all probability they constituted in pleistocene times one continuous and homogeneous district with the Arctic land, a province marked by a similar fauna and flora. This, again, is confirmed by such facts as the occurrence of

the great sea-cows which Steller discovered in Berings Island in a latitude far away from all their relatives and congeners. They are essentially animals which thrive under temperate or sub-tropical conditions, and this colony so lately occurring in the Pacific seems to point very clearly to the climate of that region having been recently milder. The same conclusion as I have previously argued seems to follow if we are to find any rational explanation of the problem of the northern migrating birds—birds which pass their summer in the Arctic and sub-Arctic lands and are scattered far and wide during the winter. Their types are so essentially northern that we can hardly doubt that at no remote time they were living all the year round in their present breeding quarters in Greenland and Spitzbergen no less than in Northern Siberia. The cold which has deprived these birds of any suitable food and resting-place in the winter is the cold now prevailing, and not a much more severe cold dating from the pleistocene age; and in explaining the difficulties and paradoxes of the distributions of animal life in the northern hemisphere, we must, as in the case of the vegetable life, invoke not a portentous period of ice in the last age of the world, but an increasing cold from the time of the mammoth onwards, and culminating in our own day.

It is clear, therefore, that Mr. Geikie and others who have drawn attractive maps showing how, during the so-called glacial epoch, the supposed glacial conditions were to be found over a large part of the polar region, will have to revise their cartography, and to erase the polar area from their maps as they already have erased the half circle of northern lands ranging from the White Sea to the eastern frontiers of Alaska, and to limit their glacial phenomena in the northern hemisphere to the two areas of North-western Europe and North-eastern America. A corollary from this, which I shall further press later on, is that in the so-called pleistocene period the fauna and flora of the whole circumpolar or Holarctic region was much more homogeneous than now, and the chief cause which has differentiated it and formed three minor provinces of it, namely, the Polar, the Palæarctic and Nearctic, has been the introduction of a much more rigorous climate in very high latitudes, which has killed off a considerable number of

forms, both of animals and plants, from the polar area, and has caused a breach of continuity and consequent isolation of the Palæarctic and Nearctic regions, with the usual result of gradually evolving local differences in Northern America and Northern Europasia, and thus constituting the Nearctic and Palæarctic zoological regions respectively.

The true and the only glacial climate which we know to have prevailed in the Arctic lands was not during the so-called glacial age of geologists—that is, during the pleistocene period; but is that which is now current, and which is the product largely, as we have seen, if not entirely, of changes of level in the earth's crust in very high latitudes which have occurred there since pleistocene times. These conclusions, if sustained, ought to throw some light on the problem of finding a rational explanation of the so-called glacial phenomena of the temperate zones, and notably of North-western Europe and North-eastern America, to which I hope to turn in the next chapter.

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